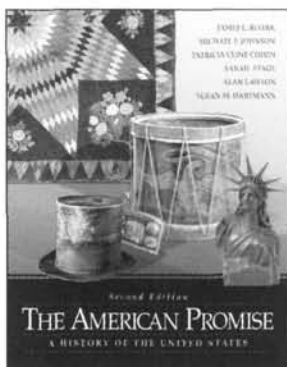


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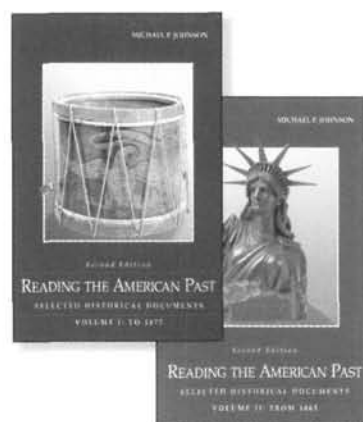
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In This Issue

This issue contains two articles, an *AHR Forum*, and a review essay. The articles offer a transnational analysis of the Irish famine and an assessment of nineteenth-century Japanese attempts to imitate Western imperialists. The *Forum* is intended to spark debate about how to understand and evaluate the development of a global economy over the last few centuries. And the review essay examines the emergence of a new media history informed by recent theory. In addition, the issue contains our usual array of book and film reviews.

Articles

Tyler Anbinder examines the great Irish famine that began in 1845 and the unprecedented emigration that it precipitated. He argues that most studies of the famine consider the story either solely from the Irish perspective and make no attempt to follow the emigrants to their new home or from the American, English, or Australian perspective and give the reader little sense of how the immigrants' lives changed after they arrived in their adopted lands. Anbinder bridges this historiographical divide by analyzing the circumstances and fate of the impoverished tenants of Lord Lansdowne's vast County Kerry estate. He details their miserable living conditions both before and during the famine. And then he tracks the Lansdowne immigrants to New York's infamous "Five Points" district, renowned as the most decrepit and dangerous neighborhood in all of North America. He finds that although the Lansdowne immigrants generally took the lowest-paying and least desirable jobs in the city, and lived in Five Points' most squalid tenements, they were able to squirrel away substantial sums in their bank accounts. Anbinder concludes that scholars must begin to reassess their previously pessimistic accounts of the famine immigrants and consider the possibility that even the most impoverished Irish famine victims living in North America's slums were able to improve their lives quickly and dramatically once they relocated to the United States. His article thus demonstrates the analytical benefits of transnational histories and the new immigration history that seeks to link people's experiences in the Old and New Worlds.

Robert Eskildsen argues that Japan's expedition to Taiwan in 1874 and the way that commercial publications represented it shows that that mimesis of Western imperialism took place as part of the process of nation building and Japan's engagement with the West. He explains that the Japanese government sent the expedition to Taiwan in part to punish a group of aborigines there but also to establish colonies. It justified colonization on the grounds of bringing civilization to the "savages" who lived there. Commercial publications, however, did not take up the government's legalistic justification of colonization. Instead, their explanations accentuated the aborigines' savagery and justified Japanese dominance over the aborigines with a variety of analogies to domestic political and social hierarchies and to Japan's own troubled encounter with the West. In doing so, Eskildsen maintains, they claimed a higher status for Japan in the Western-dominated international order and thus revealed a tendency to respond to an emerging domestic political order and the nation's new engagement with the West by preferring dominance over other East Asian peoples versus solidarity with them. The publications thus demonstrate, he concludes, that the mimetic imperialism of the expedition bore a close relationship to the Japan's broader adaptive response to Western civilization and to its efforts to build a modern nation state. Eskildsen's essay thus challenges our understanding of imperialism as a popular and cultural phenomenon as well as the relationship between state creation and empire.

AHR Forum

The *Forum* is a multifaceted examination of the history of the global economy. **Patrick Manning** introduces the subject by explaining that the essays in the *Forum* emphasize linkages among regions, not simply comparisons of wealth. As a result, they stress the strength of early modern Asian economies and argue that only in the nineteenth century did European economies pull ahead in all levels of output and productivity. Manning contends that their arguments represent a paradigm shift in our understanding of the world economy. And yet Manning urges the authors to make explicit their choices among various models for analyzing the world economy. The first author, **Kenneth Pomerantz**, reports on recent research that suggests many important economic indicators were surprisingly similar for China and Europe, even in the late eighteenth century. The efficiency of markets, changing patterns of work and consumption, and, most surprisingly, the degree of environmental strain in core regions were all similar. Consequently, he maintains, just as scholars are now much less inclined to infer from Britain's nineteenth-century economic lead that other economies in eighteenth-century Western Europe were blocked, a similar revision is now due for parts of East Asia. Pomerantz develops these contentions with an explanation of why the economic fortunes of Britain and the Yangzi Delta did ultimately diverge. He does so by emphasizing contingent factors that broke an ecological impasse potentially common to East and West in an attempt to restore a sense of discontinuity to the industrial revolution that has been lost in recent analyses. **R. Bin Wong** continues the discussion by comparing patterns of economic expansion in Asia and Europe during the early modern period. He

contends that broadly parallel dynamics of commercial expansion were embedded in distinctive political economies and suggests its significance through a counterfactual assessment of China. He then compares the characteristics of early modern international trade to those that have emerged since the mid-nineteenth century to argue that European dominance is a recent phenomenon. Wong argues that until historians are better able to delineate the similarities and differences among economies in different historical eras, it will remain difficult to craft persuasive explanations of how and when parts of Europe did in fact embark upon a distinctive path of economic growth. **David Ludden** concludes the *Forum* by suggesting that it should compel historians to take a fresh look at the origins of global inequality. He explains that Pomeranz and Wong demonstrate that the great modern divergence, which thrust China into the ranks of poor countries and Europe into the ranks of the rich, did not emerge only from indigenous sources. It began after 1800 in ways that implicate the entire world and constructed distinctively modern inequalities inside rich and poor countries as well as across national boundaries. Ludden argues that the processes by which intra-national and inter-national divergence influenced one another are a major problem for historical research and offers examples from South Asia to demonstrate the importance of these issues. He concludes by insisting that because the great divergence implicates historians around the world and has conditioned historical writing about pre-modern history at every stage, historians in rich countries must take it much more seriously.

Review Essay

Michael Schudson reviews several important new works on the history of journalism in various times and places. He argues that this emerging scholarship reveals a growing interest in media history that has been stimulated and in significant measure organized by theorists who place the media front and center in their broad interpretations of modern history. These theorists, most notably Benedict Anderson and Jürgen Habermas, focus on different aspects of the news. Habermas sees it as the raw material for rational public discourse in a liberal society; Anderson sees it as a central part of the public construction of particular images of self, community, and nation. Consequently, where Habermas connects the study of news to scholarship on politics and democratic theory, Anderson ties it to other studies of the literary or artistic products of the human imagination. Weaving between these broad perspectives and the specifics of the books at hand, Schudson suggests that Habermasian-inspired histories do not give credit to the liberalizing and democratizing influence of commercial motives and commercial news organizations. And he contends that Anderson-influenced histories of how the news media contribute to a sense of nationhood fall short of the conceptual resources (that might be found in Habermas) for distinguishing what contributes to nationalism from what contributes to the formation of liberal and democratic institutions. Schudson's essay thus helps us understand the rise of a new kind of media history that explores the concrete processes through which the press became commercialized and professionalized in particular historical contexts around the globe.



NEW YORK CITY.—IRISH DEPOSITORS OF THE EMIGRANT SAVINGS BANK WITHDRAWING MONEY TO SEND TO THEIR SUFFERING RELATIVES IN THE OLD COUNTRY.—SEE PAGE 27.

From *Frank Leslie's Illustrated Newspaper* (March 13, 1880), p. 29: "New York City.—Irish depositors of the Emigrant Savings Bank withdrawing money to send to their suffering relatives in the old country." Courtesy of the Library of Congress.

From Famine to Five Points: Lord Lansdowne's Irish Tenants Encounter North America's Most Notorious Slum

TYLER ANBINDER

AS NEW YORKER ELLEN HOLLAND looked back over her first forty-seven years of life in 1860, she must have wondered whether she was blessed or cursed. "Nelly" had been born and raised in southwestern Ireland in the County Kerry parish of Kenmare. There she grew up surrounded by jagged mountain peaks and lush green hills that sloped dramatically to the wide, majestic Kenmare River. Nelly and her family were tenants of the marquis of Lansdowne, whose estate was home to 13,000 of the most impoverished residents of nineteenth-century Ireland. Visitors to the huge property commonly chose terms such as "wretched," "miserable," "half naked," and "half fed" to describe the poor farmers and laborers who dominated its population.¹

Observers invoked such descriptions of Nelly's birthplace even *before* 1845, when a mysterious potato blight began to wreak havoc on the meager food supply. By late 1846, Kenmare residents began to succumb to starvation and malnutrition-related diseases. As conditions continued to deteriorate in early 1847, the death toll multiplied. An Englishman who visited the town of Kenmare at this time wrote that "the sounds of woe and wailing resounded in the streets throughout the night." In the morning, nine corpses were found in the village streets. "The poor people came in from the rural districts" in such numbers, wrote this observer, "it was utterly impossible to meet their most urgent exigencies, and therefore they came in literally *to die*." Tens of thousands fled Ireland in 1847, but almost none of the Lansdowne tenants could afford to emigrate. Relatively few had journeyed from this isolated estate to America in the pre-famine years, so they did not receive the remittances from abroad that financed the voyages of many famine emigrants leaving other parts of Ireland.²

Although an abatement of the potato fungus in 1848 led British officials to declare the emergency over, such decrees meant nothing to Holland and others suffering in Kenmare. Most of Lansdowne's tenants were too weak to work or plant

I would like to thank Cormac O'Grada, Gerard J. Lyne, Richard Stott, Kevin Kenny, and Patrick Williams for commenting on earlier drafts of this essay.

¹ For the source of these quotations, see nn. 23 and 24 below.

² William Bennett, *Narrative of a Recent Journey of Six Weeks in Ireland* (London, 1847), 127–29. For the low emigration rate from Kerry during the famine, see S. H. Cousens, "The Regional Pattern of Emigration during the Great Irish Famine, 1846–1851," *Institute of British Geographers Transactions and Papers*, no. 28 (1960): 121.

and too destitute to buy seed potatoes. And what few tubers they did cultivate in 1849 were again ravaged by the dreaded fungus. Kenmare once more became the center of suffering in the region, with people “dying by the dozens in the streets.” Those on the brink of death crowded into the village workhouse, where, in return for giving up all of their worldly possessions, the starving received just barely enough food to keep them alive. By April 1849, the institution held 1,800 souls “in a house built for 500—without shoes, without clothes, in filth, rags and misery,” wrote Kenmare’s Roman Catholic archdeacon, John O’Sullivan. “The women squatted on the ground, on the bare cold clay floor and [were] so imprisoned for months . . . without as much as a stool to sit on.” One of these poor souls was Ellen Holland. She and her three sons, thirteen-year-old James, nine-year-old Thomas, and four-year-old George, were almost certainly among the institution’s inmates by that point. Her husband Richard remained outside the workhouse hoping to find employment. Or he may have been one of the hundreds of men authorities turned away, both for want of space and on the theory that men could more readily find paying work than women.³

Securing one of the coveted places in the Kenmare workhouse did not ensure survival. Hundreds died there during the famine from diseases that spread rapidly in the crowded, unsanitary institution. The food supply was so meager that some inmates died of starvation-related illnesses just hours after leaving the facility. Holland likely remained in the workhouse throughout 1849 and 1850, wondering how her life might ever return to normal, or if she and her sons would also fall victim to the unending cycle of disease and death. Nelly must have been elated, then, when Lansdowne’s estate agent announced in December 1850 that the marquis would finance the emigration of all his workhouse tenants who wished to depart. Holland and her sons were among the first to take advantage of the offer.

Her jubilation soon became tinged with despair, however, as the difficulties of the journey for such emaciated, ill-equipped voyagers became more apparent. Sailors were horrified when they first encountered the Lansdowne emigrants, reporting that in the half-decade since the onset of the famine they had never laid eyes on such wretched beings. The emigrants continued to suffer as they made their way across the Atlantic. The rags they wore provided woefully inadequate protection from the elements aboard a North Atlantic sailing ship in the dead of winter. Holland’s vessel, the *Montezuma*, had to detour around an iceberg and huge swaths of “field ice” during its voyage, giving some indication of the frigid conditions she and her shipmates endured. And although Lansdowne’s agent had paid for the emigrants’ tickets, he did not supply his charges with the foodstuffs that the typical Irish emigrant brought on a transatlantic voyage. Subsisting on just one pound of flour or meal and thirteen ounces of water each day during thirty-nine

³ Gerard J. Lyne, “William Steuart Trench and the Post-Famine Emigration from Kenmare to America, 1850–1855,” *Journal of the Kerry Archaeological and Historical Society* 25 (1992): 72, 97. The composition of Holland’s family is based on the passenger manifest of the vessel that carried them to the United States. See Ira A. Glazier, ed., *The Famine Immigrants: Lists of Irish Immigrants Arriving at the Port of New York, 1846–1851*, 7 vols. (Baltimore, 1983–86), 6: 629. My belief that Holland must have been in the workhouse by late 1849 is based on the Lansdowne agent’s later statement that he chose as the first emigrants those who had been in the workhouse the longest. Because Holland was one of the first to leave under Lansdowne’s emigration program, she was probably in the workhouse by late 1849.

days at sea compounded the suffering that Holland and her Lansdowne shipmates had already endured at home.⁴

But Nelly was a strong woman, determined to build a better life for her family. Like most of the Lansdowne immigrants, she settled in New York's "Five Points" neighborhood, the most infamously decrepit slum in North America. There, surrounded by drunks, brothels, and other Irish immigrants, and living in one of the most squalid blocks of tenements in the world, Holland and her family set to work rebuilding their lives. After years of unemployment, they must have been eager and delighted to take even the lowly jobs available to them. Her husband Richard found work as a menial day laborer. Ellen became a washerwoman. The boys undoubtedly pitched in as well, for when Ellen opened an account at the Emigrant Savings Bank in September 1853, thirty months after her arrival in New York, she was able to deposit a substantial sum, \$110, equivalent to about \$2,350 today.⁵

Despite having accrued this significant nest egg in a relatively short period, Holland's struggles continued. By July 1855, both her husband and eldest son were dead. One might have expected her to dip into her savings to help make ends meet during such trying times, but Nelly did no such thing. In fact, despite losing her family's two primary breadwinners, by 1860 she had increased her bank balance to \$201.20 (more than \$4,200 today), a real feat for a widow who, just eight years earlier, had been on the brink of starvation and had lived the first thirty-eight years of her life in a land of chronic underemployment and hunger. More surprising still, among the hundreds of Lansdowne immigrants who came to New York—most, like Holland, arriving utterly destitute—such relative financial success was not all that unusual.⁶

Hundreds of thousands of men and women like Ellen Holland emigrated from Ireland to North America during the famine years. Yet, dramatic as her story may be, few such tales can be found in the historiography of the famine immigration to America. This has resulted to a large extent from the divided nature of Irish studies,

⁴ *New York Herald*, March 17, 1851 (report of the *Montezuma's* voyage and arrival).

⁵ My calculation of the current value of Holland's savings is based on the multiplier suggested by the U.S. Department of Labor's *Handbook of Labor Statistics*, available on the World Wide Web at <http://minneapolisfed.org/economy/calc/hist1800.html>, which suggests a multiplier of 21.34 to convert 1850 dollars into 2001 dollars. According to the Department of Labor's statistics, a dollar in 1860 was worth about the same amount in real terms as a dollar in 1850, due to the deflationary effects of the panics of 1854 and 1857. The department's figures are borne out by John J. McCusker, "How Much Is That in Real Money?" *Proceedings of the American Antiquarian Society* 101 (1991): 327–32, which suggests a multiplier of 16 to convert dollar amounts from the 1850s into 1991 dollars. Adjusting McCusker's figure to take into account inflation since 1991 (using the Consumer Price Index calculator at <http://minneapolisfed.org/economy/calc/cpihome.html>) brings virtually the identical result. All subsequent estimates of the current value of nineteenth-century monetary figures are based on the Department of Labor's conversion program. It is, I admit, very difficult to know whether or not to trust these conversion systems. They do not produce consistently satisfactory results. Nonetheless, I feel it is important to offer estimates, because without them, the monetary figures from the nineteenth century are meaningless to most modern readers. These estimates of the modern value of the Emigrant Savings Bank account balances are different from (and should be used in place of) those appearing in my book *Five Points* (New York, 2001). At the time *Five Points* went to press, I did not fully appreciate the impact of 1990s inflation on the Emigrant Savings Bank account information.

⁶ Accounts 5479 and 9445, Test Books and Account Ledgers, Emigrant Savings Bank Collection, New York Public Library. The bank collected genealogical information in its "test books" because a person wishing to make a withdrawal had to pass a biographical test in order to prove that he or she was truly the account holder.

as the best works in the field treat either the Irish or the American story but rarely follow the emigrants from Europe to America. A number of fine books have examined the conditions that drove about 2 million Irishmen to flee the Emerald Isle in the famine years, but these studies do not offer detailed accounts of their subjects' fates in America.⁷ The best works on the Irish in the United States likewise devote relatively little attention to Irish Americans' lives before they immigrated. David M. Emmons in *The Butte Irish* and Kevin Kenny in *Making Sense of the Molly Maguires* trace their protagonists to West Cork and West Donegal respectively, but the reader gets little sense of how individual lives changed.⁸ Studies of famine-era Irish immigration to Canada and England have followed the same historiographic pattern.⁹

Given the current trend toward "internationalizing" our study of history (both the *American Historical Review* and the *Journal of American History* have recently focused attention on this subject), one might imagine that studies of the Irish diaspora would have begun to compare their subjects' pre- and post-emigration lives more fully. Those who study the seventeenth and eighteenth-century "Atlantic World," for example, have produced a number of sophisticated and influential monographs that cross national boundaries, but this trend has had little impact on historians of immigration, the vast majority of whom study the nineteenth and twentieth centuries.¹⁰ Most sociologists, in contrast, have warmly embraced the concept of "transnationalism," arguing that modern means of communications and transportation have created a new breed of immigrant who simultaneously main-

⁷ Kerby A. Miller, *Emigrants and Exiles: Ireland and the Irish Exodus to North America* (New York, 1985); James S. Donnelly, Jr., *The Land and the People of Nineteenth-Century Cork* (London, 1975); Robert J. Scally, *The End of Hidden Ireland: Rebellion, Famine, and Emigration* (New York, 1995); Ciarán Ó Murchadha, *Sable Wings over the Land: Ennis, County Clare, and Its Wider Community during the Great Famine* (Ennis, Ireland, 1998).

⁸ David M. Emmons, *The Butte Irish: Class and Ethnicity in an American Mining Town, 1875–1925* (Urbana, Ill., 1989); Kevin Kenny, *Making Sense of the Molly Maguires* (New York, 1998).

⁹ For Canada, see Cecil J. Houston and William J. Smyth, *Irish Emigration and Canadian Settlement: Patterns, Links, and Letters* (Toronto, 1990); Donald McKay, *Flight from Famine: The Coming of the Irish to Canada* (Toronto, 1990); Thomas P. Power, ed., *The Irish in Atlantic Canada, 1780–1900* (Fredericton, New Brunswick, 1991); Robert J. Grace, *The Irish in Quebec: An Introduction to the Historiography* (Sainte-Foy, Quebec, 1997). The literature on the Irish in England is far more vast. Among the best works are Lynn Hollen Lees, *Exiles of Erin: Irish Migrants in Victorian London* (Ithaca, N.Y., 1979); Frances Finnegan, *Poverty and Prejudice: A Study of Irish Immigrants in York, 1840–1875* (Cork, 1982); Roger Swift and Sheridan Gilley, eds., *The Irish in the Victorian City* (London, 1985); Swift and Gilley, eds., *The Irish in Britain, 1815–1939* (London, 1989); Swift and Gilley, eds., *The Irish in Victorian Britain: The Local Dimension* (Dublin, 1999); W. J. Lowe, *The Irish in Mid-Victorian Lancashire: The Shaping of a Working-Class Community* (New York, 1989); Donald MacRaild, *Culture, Conflict, and Migration: The Irish in Victorian Cumbria* (Liverpool, 1998).

¹⁰ AHR Forum: "Crossing Slavery's Boundaries," *AHR* 105 (April 2000): 451–84; "The Nation and Beyond," a special issue on "transnational history," *Journal of American History* 86 (1999): 965 and following. The best-known practitioner of Atlantic history is Bernard Bailyn, both through his book *Voyagers to the West: A Passage in the Peopling of America on the Eve of the Revolution* (New York, 1986) and through his long-running seminar on the Atlantic world. See Bailyn, "The Idea of Atlantic History," Working Paper 96–01, International Seminar on the History of the Atlantic World, 1500–1800. See also David Hancock, *Citizens of the World: London Merchants and the Integration of the British Atlantic Community, 1735–1785* (New York, 1995); W. Jeffrey Bolster, *Black Jacks: African American Seamen in the Age of Sail* (Cambridge, 1997); Alison Games, *Migration and the Origins of the English Atlantic World* (Cambridge, 1999); and one study related to Ireland, Nicholas P. Canny, *Kingdom and Colony: Ireland in the Atlantic World, 1560–1800* (Baltimore, 1988).

tains strong ties to two lands.¹¹ Historians, justifiably skeptical about claims that contemporary immigrants are so different from their predecessors, have been reluctant to jump onto the transnational bandwagon. Those interested in immigration and ethnicity have instead focused on issues such as “whiteness” (exemplified by Noel Ignatiev’s provocative *How the Irish Became White*), nativism, and other aspects of cultural history.¹² Research on the Irish elsewhere has followed the same trends, although a recent survey of the field in England lamented that “the academic study of the Irish in Britain continues to lag far behind its counterpart in the United States.” Even the appearance of a six-book series entitled “The Irish World Wide,” while significant, did not portend any sea change in Irish historiography, as virtually every essay in the collection looked at the Irish in a single town or city. The 150th anniversary of the famine did produce a surge in publications on that subject, but little that would help explain whether the relative financial success of Ellen Holland and her friends was typical or exceptional.¹³ And there are no signs of a revival in the kinds of “mobility studies,” pioneered more than thirty years ago by Stephan Thernstrom, which might enable us to put the financial achievements of someone like Ellen Holland into context.¹⁴

Those familiar with Irish and Irish-American historiography would certainly suppose that Ellen Holland’s story must be unusual. A deep pessimism has pervaded this literature, assuming that the famine immigrants were a kind of lost generation fated to be victims of disease, nativism, low-paying jobs, and overcrowded tenements in North America or England. Any significant improvement in their circumstances, such studies imply, came in the lives of their assimilated children. There are exceptions, but most scholars continue to believe, as Oscar Handlin put it more than fifty years ago in *Boston’s Immigrants*, that the famine

¹¹ A concise introduction to the concept of transnationalism is Alejandro Portes, “Global Villagers: The Rise of Transnational Communities,” *The American Prospect* 25 (March–April 1996): 74–77. The literature in this field is voluminous, but for a representative sample see Luis Guarnizo and Michael P. Smith, eds., *Transnationalism from Below* (New Brunswick, N.J., 1997); Nancy Foner, “What’s New about Transnationalism? New York Immigrants Today and at the Turn of the Century,” *Diaspora* 6 (1997): 355–75; Eduardo Guarnizo, et al., “Mistrust, Fragmented Society, and Transnational Migration: Colombians in New York City and Los Angeles,” *Ethnic and Racial Studies* (United Kingdom) 22 (March 1999): 367–96.

¹² Noel Ignatiev, *How the Irish Became White* (New York, 1995); David R. Roediger, *The Wages of Whiteness: Race and the Making of the American Working Class* (New York, 1991); Eric Lott, *Love and Theft: Blackface Minstrelsy and the American Working Class* (New York, 1993).

¹³ Swift and Gilley, *Irish in Victorian Britain*, 13 (quotation); Patrick O’Sullivan, ed., *The Irish World Wide*, 6 vols. (London, 1992–96). Among the best of the recent books on the famine are Christine Kinealy, *This Great Calamity: The Irish Famine, 1845–1852* (Dublin, 1994); Cormac O’Grada, *Black ’47 and Beyond: The Great Irish Famine in History, Economy, and Memory* (Princeton, N.J., 1999); and Peter Gray, *Famine, Land, and Politics: British Government and Irish Society, 1843–1850* (Dublin, 1999).

¹⁴ Stephan Thernstrom, *Poverty and Progress: Social Mobility in a Nineteenth-Century City* (Cambridge, Mass., 1964); Thernstrom, *The Other Bostonians: Poverty and Progress in the American Metropolis, 1880–1970* (Cambridge, Mass., 1973); Peter R. Knights, *Plain People of Boston: A Study in City Growth, 1830–1860* (New York, 1971); Clyde and Sally Griffen, *Natives and Newcomers: The Ordering of Opportunity in Mid-Nineteenth-Century Poughkeepsie* (Cambridge, Mass., 1978); Thomas Kessner, *The Golden Door: Italian and Jewish Immigrant Mobility in New York City, 1880–1915* (New York, 1977). Little mobility work has focused on the antebellum Irish, but in addition to the work by Knights and the Griffens, see Jo Ellen McNergney Vinyard, *The Irish on the Urban Frontier: Nineteenth-Century Detroit, 1850–80* (New York, 1976); and Dale Light, “Class, Ethnicity, and the Urban Ecology: Philadelphia’s Irish, 1830–1880” (PhD dissertation, University of Pennsylvania, 1979).

Irish were both economically and socially “fated to remain a massive lump in the community, undigested, undigestible.”¹⁵

When I began investigating the history of Five Points, I assumed that the prevailing, gloomy picture of the famine-era immigrants would be borne out on its mean streets. Given that Five Points’ residents were the most impoverished in antebellum New York, I expected to find them barely scraping by from payday to payday. I was especially sure that the Lansdowne immigrants would fit this stereotype. They made up about one in nine of Five Points’ Irish-Catholic inhabitants in the 1850s and were concentrated overwhelmingly in the most squalid tenements in the neighborhood’s most decrepit and crime-ridden blocks. But the bank balances of Ellen Holland and her fellow Lansdowne immigrants force us to reconsider such long-held preconceptions.¹⁶

IT WAS AN ENGLISH NOBLEMAN, the third marquis of Lansdowne, who, by deciding to finance a massive Irish emigration program, made it possible for Ellen Holland and about a thousand of her friends, family members, and neighbors to move to Five Points. Born Henry Petty in about 1780, Lansdowne from an early age took an interest in British politics. He considered himself an “independent Whig” and as a young man entered the Cabinet, serving as chancellor of the Exchequer at twenty-six as a result of his expertise in political economy and financial and administrative theory. At a time when English aristocrats commonly supported artists by allowing them to live on their estates and paint at their leisure, Lansdowne did the same for economists, specifically those of a classically liberal bent. So many of them lived at or visited Bowood House, his Wiltshire mansion, that the leading economists of the era who espoused free trade and complete laissez faire became

¹⁵ Oscar Handlin, *Boston’s Immigrants: A Study in Acculturation*, rev. edn. (Cambridge, Mass., 1959), 55. Two notable exceptions to the pessimistic tenor of famine-era Irish-American historiography are Hasia R. Diner, *Erin’s Daughters in America: Irish Immigrant Women in the Nineteenth Century* (Baltimore, 1984), who emphasizes women’s success (primarily as live-in domestic servants) at earning money to send to their families in Ireland; and Joseph P. Ferrie, *Yankees Now: Immigrants in the Antebellum United States, 1840–1860* (New York, 1999), who finds significant upward mobility for antebellum immigrants. For an optimistic portrayal of the Irish who settled in England, see Donald M. MacRaild, *Irish Migrants in Modern Britain, 1750–1922* (London, 1999).

¹⁶ Irish immigrants made up 66 percent of the adult Five Points population in 1855, according to the state census of that year. The marriage records of the neighborhood’s Roman Catholic church, the Church of the Transfiguration, whose secretary noted the county and parish of birth of nearly every person married there from 1853 to 1859, indicate that 14 percent of Irish-Catholic immigrant Five Pointers were natives of County Kerry. Those records show that 79 percent of the neighborhood’s Kerry residents were natives of the Lansdowne estate, leading to my assertion that one in nine Irish-Catholic Five Points adults in the mid-1850s was a Lansdowne immigrant. My figures probably approximate the county origins of all Five Points Irish immigrants fairly accurately, because although there were some Irish Protestants in the neighborhood, there were not many. This is confirmed by an analysis of the Emigrant Savings Bank Test Books, which also listed place of birth and whose depositors included Protestants and Jews as well. Of the Five Pointers in the Transfiguration register, 173 were from Sligo, 142 from Cork, 141 from Kerry, 59 from Galway, 56 from Limerick, 52 from Tipperary, 42 from Mayo, 36 from Leitrim, 40 from Roscommon, 30 from Waterford, 27 from Kilkenny, 19 from Dublin (city and county), 17 from Tyrone, 16 from Donegal, 15 from Fermanagh, 15 from Clare, 15 from Longford, 14 from Meath, 14 from Louth, 13 from Queen’s (now Laois), 12 from Westmeath, 12 from King’s (now Offaly), 12 from Cavan, 10 from Monaghan, 9 from Wexford, 9 from Armagh, 9 from Derry, 8 from Kildare, 7 from Carlow, 3 from Down, 3 from Wicklow, and none from Antrim. See Marriage Register, Church of the Transfiguration, 29 Mott Street, New York.

known as the “Bowood set.” Though a failure as chancellor of the Exchequer, Lansdowne remained an influential figure in English politics throughout his life, serving in various Whig cabinets and for many years as Lord President of Her Majesty’s Council.¹⁷

Lansdowne owned 95,000 statute acres in various parts of west Kerry, but the bulk of his holdings (and those from which all of his assisted emigrants originated) was in the southwest corner of the county in the barony of Glanarought. Consisting of about 67,000 statute acres (105 square miles), Lansdowne’s Glanarought estate spanned nearly three civil parishes—all of Tuosist, the Kerry portion of Kilcascan, and more than half of Kenmare. This vast property, one of the dozen largest estates in nineteenth-century Ireland, had been a mid-seventeenth-century gift to Sir William Petty for his role in subduing the Irish “rebels” who had resisted England’s conquest of the island. Petty’s descendants eventually received peerages, becoming the marquises of Lansdowne.¹⁸

Lansdowne’s Irish estate (this and all subsequent references to the “Lansdowne estate” refer to the Glanarought portion of his holdings) was “a very rocky and mountainous country.” A topographical guide to Ireland described Tuosist, the largest of the three parishes, as “one of the wildest and most irreclaimable districts in the county; it is separated on the south-east from the county of Cork by a range of lofty and almost impassable mountains.” Kilcascan was still more remote than Tuosist, for while the latter parish included a long stretch of the south shore of the Kenmare River (really a bay at this point), Kilcascan sat entirely within the mountains, and as late as 1837 neither of the two main roads in “this wild and mountainous district” was passable by carriage. Residents usually referred to this isolated area as “Bonane” (the name of the Catholic parish it encompassed) rather than by the civil classification of Kilcascan. “Rocky mountain and bog” characterized Kenmare parish as well, although it did contain the estate’s one sizable village—the town of Kenmare—with a population of 1,300 in 1841. The large majority of the estate that lay south of the wide river was thoroughly isolated from Kenmare until 1842, just before the famine, when a suspension bridge was completed to link “the wilds of Tuosist” and Bonane to Kenmare town and the roads to Cork and Killarney.¹⁹

Only a tiny fraction of this vast district was arable. “The portions of land reclaimed from the rocky mountains . . . are so small,” wrote the Lansdowne agent

¹⁷ Harriet Martineau, *Biographical Sketches* (New York, 1869), 329–37; Peter Mandler, *Aristocratic Government in the Age of Reform: Whigs and Liberals, 1830–1852* (London, 1990), 28–35, 101–04, 173–74. The first modern biographical study of Lansdowne, by Professor John Powell of Cumberland College, will be published in 2003 in *Parliamentary History*.

¹⁸ On the history and extent of Lansdowne’s holdings, see Gerard J. Lyne, *The Lansdowne Estate in Kerry under the Agency of William Steuart Trench, 1849–72* (Dublin, 2001), xvii–xxii, xxx–xxxviii, lviii–lxii; [William] G. Carroll, *The Lansdowne Irish Estates and Sir William Petty*, 2d edn. (Dublin, 1881), 4, 9–11, 21; Samuel Lewis, *A Topographical Dictionary of Ireland*, 3 vols. (1837; rpt. edn., Port Washington, N.Y., 1970), 1: 230–31, 2: 37–38, 661; *The Times* (London), January 7, 1881 (for its status as one of the largest estates in Ireland); *Report of the Commissioners Appointed to Take the Census of Ireland for the Year 1841* (Dublin, 1843), 198.

¹⁹ House of Commons, *Evidence Taken . . . in Respect to the Occupation of Land in Ireland*, 1845 [616], 20: 910; Lewis, *Topographical Dictionary of Ireland*, 1: 230–31, 2: 37–38, 661; [Sixth] Marquis of Lansdowne, *Glanerought and the Petty-Fitzmaurices* (London, 1937), 125–26 (suspension bridge); Lyne, “Post-Famine Emigration from Kenmare,” 133 (“wilds of Tuosist”).

in 1869, “that they are barely sufficient to grow potatoes and turnips enough for the consumption of the people.” Most of the Lansdowne property was instead used as pasture for cows and sheep. As a result, the estate’s population on the eve of the famine was relatively modest—only about 12,800 people—of which 7,500 lived in Tuosist, 3,900 in Kenmare parish, and 1,400 in Bonane. Yet despite being spread out over a wide area, Lansdowne’s tenants were a close-knit bunch. Tuosist priest Callaghan McCarthy told a visitor that “there existed considerable remains of clanship among these mountaineers. He described them as highly moral, a careless, but a peaceable and contented race, with great kindness and simple hospitality, and strong family attachments.”²⁰

These “mountaineers” were among the most destitute inhabitants of Ireland. Laborers were lucky to find more than a day or two of paid work in a week, and with such an oversupply of unskilled workers, wages fell to incredibly low levels. With so little arable land available, rents for potato land skyrocketed, meaning that most small-scale, impoverished farmers and laborers could neither grow nor buy enough food for their families. James Hickson, the Lansdowne agent until mid-century, testified that the typical poor man’s diet consisted entirely of either “potatoes and milk, or potatoes and fish; some are so poor as to use [only] potatoes and salt.” Another source confirmed that “no groceries are used in a labourer’s family except a very little at Christmas.” Many impoverished families had chickens that laid eggs, but they never ate them, selling them instead to pay for tobacco. Summer months were particularly difficult, as the previous year’s potato supply ran out or spoiled weeks or months before the new crop was ready for harvest. “Hungry July” was a common phenomenon throughout western Ireland, but especially so on the Lansdowne estate.²¹

Lansdowne tenants were so desperately poor that they would often nail shut their cabins during the summer and walk a hundred miles or more through the counties of Cork, Limerick, or Tipperary in search of work. “In autumn they go to the low country during the harvest,” noted a Kerry resident, “and their wives then often shut up their houses and go begging with their families until their husbands come home with their earnings” in time to harvest their own potatoes. After digging up the tubers, some again went inland to find work before returning home for Christmas. Not all Lansdowne laborers needed to roam the countryside in search of work, but those who lived on remote mountainsides in Tuosist and Bonane could

²⁰ *Report of the . . . Census of Ireland for the Year 1841*, 198; Bennett, *Narrative of a Recent Journey of Six Weeks in Ireland*, 129–30; William S. Trench, *Realities of Irish Life* (London, 1868), 112–13.

²¹ House of Commons, “Poor Inquiry (Ireland), Appendix E, Containing Baronial Examinations Relative to Food, Cottages and Cabins, Clothing and Furniture, Pawnbroking and Savings Banks, Drinking,” *Sessional Papers, Reports from Commissioners*, 1836, 32: 58, 106 (alcohol), supplement p. 213 (Hickson); “Poor Inquiry (Ireland), Appendix D, Containing Baronial Examinations Relative to Earnings of Labourers, Cottier Tenants, Employment of Women and Children, Expenditure,” *Sessional Papers, Reports from Commissioners*, 1836, 31: 81, 89, 108; “Poor Inquiry (Ireland), Appendix F, Containing Baronial Examinations Relative to Con Acre, Quarter or Score Ground, Small Tenantry, Consolidation of Farms and Dislodged Tenantry, Emigration,” *Sessional Papers, Reports from Commissioners*, 1836, 33: 22; *Evidence Taken . . . in Respect to the Occupation of Land in Ireland*, 1845 [616], 20: 910–12, 918–19; Jonathan Binns, *The Miseries and Beauties of Ireland* (London, 1837), 2: 333–34; Kevin Danaher, *The Year in Ireland* (Cork, 1972), 163–66.

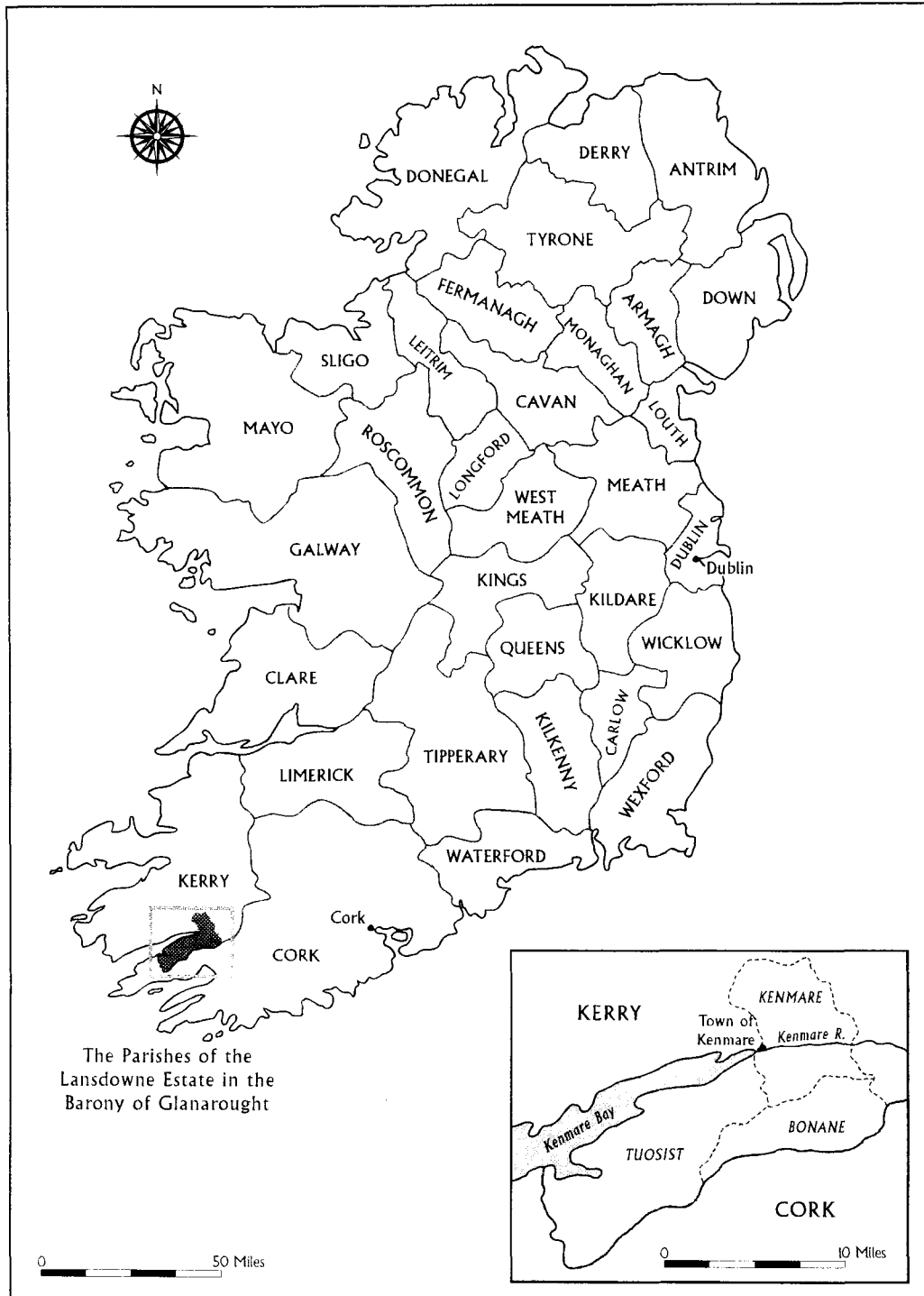


FIGURE 1: Map of Ireland, with the parishes of the Lansdowne estate highlighted. Inset shows location of Kenmare, Tuosist, and Bonane parishes within the estate. Mapmaker Chris Robinson.

rarely rent enough potato land to feed their families, and thus had no choice but to partake of this migratory ritual.²²

During the portions of the year when they inhabited them, and especially until the mid-1830s, Lansdowne's tenants lived in some of the worst dwellings in all of Ireland. Their cottages usually measured "13 feet long by 10 broad" and were

built of stone and mud. It contains generally but one apartment. The roof is merely thatch . . . of straw, heath, or potato stalks . . . The floor is made of beaten earth; it is commonly damp, being below the level of the surrounding ground, and below the water contained in the manure holes outside the door . . . There are no chimneys in general . . . Cabins never contain [cooking] grates; there is no glass in the windows . . . Many of the worst cabins have no wooden door but a kind of hurdle with heather or long grass woven among the sticks.

One visitor who traveled throughout Ireland in 1834 called these "as miserable cabins as I ever beheld." They were "beyond description, wretched abodes." Apparently embarrassed, Lansdowne began financing the construction of stone homes to replace the mud hovels. By the eve of the famine, stone cabins predominated, but the revolutionary Michael Doheny, who hid in Tuosist cabins after the failed uprising of 1848, insisted that in many cases their "showy exterior is sadly belied by the filth and discomfort of the inside." Such cabins were almost devoid of furniture, and the occupants (especially women and children) often had no clothing other than the tatters on their backs.²³

Who was to blame for this situation? Doheny faulted Lansdowne. William Steuart Trench, the marquis's agent beginning at mid-century, blamed his predecessor as well as the tenants themselves. Father O'Sullivan, who was no admirer of Lansdowne, nonetheless admitted that the marquis charged very reasonable rents and did not evict tenants even when they fell years behind in their payments. No matter whom one faulted, all observers agreed with Irish Poor Inquiry official Jonathan Binns that Lansdowne's "poor cottagers are in a very distressed condition . . . They are nearly half naked, and are but half fed. This is indeed a wretched state of things." O'Sullivan was even more blunt. Laborers on the Lansdowne estate, he asserted in 1844, were "the most wretched people upon the face of the globe."²⁴

GIVEN THEIR PRECARIOUS POSITION, it should come as no surprise that Lansdowne's tenants were devastated by the famine that commenced after a fungus began to destroy potato crops in the summer of 1845. "Kenmare was completely paralysed," asserted Trench, who recalled that "famine stalked unmolested through the glens and mountains." During the first year after the blight struck, from mid-1845 to

²² Trench, *Realities of Irish Life*, 112–13 (quotation); Archdeacon John O'Sullivan in *Tralee Chronicle*, February 9, 1850, in Lyne, "Post-Famine Emigration from Kenmare," 66; House of Commons, "Poor Inquiry (Ireland), Appendix D," 52; Henry D. Inglis, *Ireland in 1834: A Journey throughout Ireland during the Spring, Summer, and Autumn of 1834*, 2d edn. (London, 1835), 1: 209–13.

²³ House of Commons, "Poor Inquiry (Ireland), Appendix E," 58, 90, supplement p. 213; Binns, *Miseries and Beauties of Ireland*, 2: 333–34; Inglis, *Ireland in 1834*, 209–13; Michael Doheny, *The Felon's Tracks* (1849; rpt. edn., Dublin, 1951), 244.

²⁴ Trench, *Realities of Irish Life*, 112–13; *Evidence Taken . . . in Respect to the Occupation of Land in Ireland*, 1845 [616], 20: 912, 919; Binns, *Miseries and Beauties of Ireland*, 2: 336–37; Doheny, *Felon's Tracks*, 244.

mid-1846, the distress was relatively mild. Many, in fact, refused to eat the “Indian corn meal” imported from North America by Lansdowne and the Kenmare Relief Committee. With the complete failure of the potato crop in 1846, however, attitudes changed rapidly. The hungry now demanded cornmeal, noted the committee in August, while also reporting that “dysentery and the common cholera are already beginning to make their appearance to a frightful extent.” Committee members warned that starvation would be inevitable unless the government spent more on relief efforts.²⁵

Instead, expenditures on aid were cut back, and even those who did receive assistance did not get enough. By February 1847, deaths from starvation were common. “This neighbourhood is becoming depopulated with railway speed,” wrote one relief official from Kenmare, describing the public works projects set up to employ the starving laborers and cottiers (those who subsisted by renting tiny parcels of farm land):

I see nothing within the bounds of possibility that can save the people. On one road, on which I have 300 men employed, the deaths are three each day. This is in the parish of Tuosist. The people are buried without coffins, frequently in the next field. No noise or sign of grief for the dead; every thought is selfish and unfeeling . . . I daily witness the most terrible spectacles. Men and women are discolored with dropsy, attacked with dysentery, or mad with fever, on the works—driven there by the terrible necessity of trying to get as much as would purchase a meal . . . With most of these working is a mockery; they can scarcely walk to and from the roads, and how can they work! . . . When a respectable person passes the houses of these poor people, the saddest sights present themselves; women, children, and old men crawling out on all fours, perhaps from beside a corpse, to crave a morsel of any kind of food.

Men predominated among the dead because they were forced to toil on such road crews, even though the small allotments of food they received as payment could not sustain them.²⁶

Conditions in the town of Kenmare were hardly better than those in the countryside. O’Sullivan recorded in his diary in early 1847 that there was “nothing more usual than to find four or five bodies in the street every morning.” The suffering of the living was almost as difficult to bear as the sight of the dead. “The swollen limbs, emaciated countenances, and other hideous forms of disease . . . were innumerable,” gasped visitor William Bennett. “In no other part of Ireland had I seen people falling on their knees to beg. It was difficult to sit over breakfast after this.” O’Sullivan wrote directly to Charles Trevelyan, permanent secretary at the Treasury in London and the man who singlehandedly controlled most relief expenditures, hoping that if Trevelyan understood the extent of the suffering he might make more aid available. “The cries of starving hundreds that besiege me

²⁵ Lansdowne, *Glanerought and the Petty-Fitzmaurices*, 127; Trench, *Realities of Irish Life*, 113–14; Kenmare Relief Committee to the Lord Commissioners of Her Majesty’s Treasury, August 22, 1846, in “Correspondence Relating to the Measures Adopted for the Relief of Distress in Ireland (Board of Works Series) First Part,” 1847 [764], 50: 62–63, in *Irish University Press Series of British Parliamentary Papers*, “Famine” series (Shannon, 1970), 6: 94–95 (hereafter, *IUP-BPP, Famine*).

²⁶ Mr. Gill to Mr. Russell, February 25, 1847, in House of Commons, “Correspondence Relating to the Measures Adopted for the Relief of Distress in Ireland (Commissariat Series) Second Part,” 1847 [796], 52: 192, in *IUP-BPP, Famine*, 7: 550.

from morning until night actually ring in my ears during the night," O'Sullivan reported. "I attended myself a poor woman, whose infant, dead two days, lay at the foot of the bed, and four others nearly dead in the same bed; and, horrible to relate, a famished cat got up on the corpse of the poor infant and was about to gnaw it, but for my interference. I could tell you such tales of woe without end."²⁷

Conditions in Tuosist were especially bad. "It is a wild alpine region; the inhabitants being mostly self-dependent, and in ordinary times holding very little communication with the rest of the world," Bennett explained. A clergyman there told him that one-third of the inhabitants "had no other means of subsistence, at the present moment, than sea-weed and shell-fish from the rocks." Part of the problem, according to Captain Erasmus Ommanney, head of the Kenmare relief district, was that his subordinates in Tuosist were sometimes "exercising undue severity on the applicants for relief." Hundreds who could not gain admission to the already full poorhouse in Kenmare were starving to death in Tuosist, but the local poor law guardians followed the letter of the law and refused to grant relief to those living outside the workhouse.

Even when such rules were eventually waived, relief officials' hardheartedness often led to tragic results. In December 1847, Honora Connolly and her five daughters left the Kenmare poorhouse after receiving assurances that they would be eligible for food back home in Tuosist. When she arrived there, however, agents refused to provide her with food, insisting she was not on their list. "Norry" walked back to Kenmare to investigate the problem, leaving four of her five daughters behind, but by the time she returned to Tuosist the following day, all four lay dead. An investigation eventually revealed that the relief officials had mistakenly recorded Connolly's name on their list as Norry Harrington. Her four children were a few of the "thousands in Tuosist sunk beneath the sudden loss of the potatoe." Conditions there, concluded Bennett, were "utterly past the powers of description, or even of imagination, without witnessing."²⁸

With the death toll mounting, one wonders why Lansdowne did not consider financing the emigration of his tenants as a means to relieve both their suffering and *his* obligation to feed them (the government had shifted most of the burden for relief efforts to wealthy Irish landlords such as Lansdowne). By the end of 1847, his political confidante, Lord Palmerston, had already sent 2,000 of his starving County Sligo tenants to Canada. A few other prominent landlords had followed suit. Very few Lansdowne tenants could afford to emigrate on their own, and few had relatives in the United States who could finance their emigration. At the end of 1847, most

²⁷ O'Sullivan to Trevelyan, "February, 1847," in House of Commons, "Correspondence Relating to the Measures adopted for the Relief of Distress in Ireland (Commissariat Series) Second Part," 1847 [796], 52: 192, 166, in *IUP-BPP, Famine*, 7: 550, 524; Bennett, *Narrative of a Recent Journey of Six Weeks in Ireland*, 127–29; O'Sullivan Diary, c. March 1847, in Lyne, "Post-Famine Emigration from Kenmare," 125.

²⁸ Bennett, *Narrative of a Recent Journey of Six Weeks in Ireland*, 129–30, 132; Captain Erasmus Ommanney to the Commissioners, March 12, 1848, in House of Commons, "Papers Relating to Proceedings for the Relief of the Distress and State of the Unions and Workhouses in Ireland; Sixth Series," *Sessional Papers*, 1848 [955], 56: 336, in *IUP-BPP, Famine*, 3: 336; Ommanney to the Commissioners, November 28, December 12 (quotation), 1847, January 8, 1848, in House of Commons, "Papers Relating to Proceedings for the Relief of . . . Ireland; Fifth Series," 1847–48 [919], 60: 511, 514, 519–26, in *IUP-BPP, Famine*, 2: 835, 838, 843–50, 852; Trench ("sunk beneath") quoted in Lyne, "Post-Famine Emigration from Kenmare," 124–25.

of Lansdowne's tenants were still in Kerry receiving relief rations paid for primarily by the marquis himself.²⁹

Perhaps Lansdowne believed that his Kerry tenants could survive the blight without his having to expend the huge sums on emigration laid out by Palmerston. If so, reports from Kenmare in the spring of 1848 must have pleased the marquis immensely. All signs indicated that suffering and privation were on the wane, and the potatoes his tenants planted in 1848 initially showed no signs of blight.³⁰ But either because of illness or lack of seed potatoes, Lansdowne's leaseholders had not planted nearly enough potatoes to feed themselves for a whole year, and by February 1849 gruesome reports of starvation again began emanating from Kenmare. "I was shocked in Skibbereen, Dunmanway, [and] Bantry," wrote a visitor to Kenmare who had just come from those infamously destitute west Cork towns, "but they were as nothing to what was now before me . . . Bad as the Bantry paupers were they were 'pampered rogues' in comparison to these poor creatures . . . Spectres from the grave could not present a more ghastly, unearthly appearance . . . The very thought of them to this moment sickens me." The emaciated once again crowded into Kenmare, "dying by the dozens in the streets." According to O'Sullivan, "theft and robbery and plunder became . . . universal" as others used any available means to stave off starvation. However obtained, food alone did not necessarily ensure survival. The cholera epidemic sweeping Europe and North America in the spring of 1849 also struck Kenmare, and due to the overcrowding in the workhouse, its inmates were particularly susceptible. Dysentery afflicted many as well, observed O'Sullivan, its victims so thirsty that they would barter their weekly one pound relief ration of cornmeal "for a half noggin of new milk to try and quench the burning thirst which invariably follows them." Despite government declarations that the famine was over, the death toll in southwest Kerry climbed steadily higher in 1849. By the end of that year, after the blight again destroyed the 1849 crop, at least 1,000 (and perhaps as many as 1,700) of Lansdowne's 12,000 tenants had succumbed to the famine and the diseases spread in its wake. Many others were barely hanging on.³¹

In 1850, Lansdowne hired a new estate agent, William Trench, in hopes that he might better administer relief to the suffering tenants. Trench decided that only

²⁹ Lansdowne, *Glanerought and the Petty-Fitzmaurices*, 127; Ommanney to the Commissioners, March 5, 1848, in House of Commons, "Papers Relating to Proceedings for the Relief of . . . Ireland; Sixth Series," 1848 [955], 335, in *IUP-BPP, Famine*, 3: 335. For the Palmerston emigration program, see Tyler Anbinder, "Lord Palmerston and the Irish Famine Emigration," *The Historical Journal* (Cambridge) 44 (2001): 441–69.

³⁰ Ommanney to the Commissioners, March 26, April 1, 1848, in House of Commons, "Papers Relating to Proceedings for the Relief of . . . Ireland; Sixth Series" [955], 339–40, in *IUP-BPP, Famine*, 3: 339–40.

³¹ Unknown writer to "My dear William," February 27, 1849; copies of O'Sullivan to [Poulett Scrope?], April 16, 1849 (on cholera, not quoted); and O'Sullivan to Poulett Scrope, April 30, 1849; *The Nation*, December 12, 1857, all in Lyne, "Post-Famine Emigration from Kenmare," 97, 72, 100–01. The mortality figure is based on Lansdowne, *Glanerought and the Petty-Fitzmaurices*, 128–29, which cites Trench as saying that 5,000 had died in the Kenmare "union" (relief district) by the time he became agent in early 1850. The Lansdowne estate made up about one-third of the Kenmare union, thus my upper estimate that about one-third of that figure had died. O'Sullivan, in his critique of Trench's memoir, insisted that the actual figure was far lower. Given the numbers dying in the workhouse and on works projects described in the contemporary reports, it is hard to imagine that fewer than 1,000 died on the estate.

radical measures could both feed Lansdowne's tenants and make the estate profitable in the foreseeable future. In November, the agent sailed for England to describe his plan to Lansdowne in person at Bowood. As Trench recalled the discussion in his memoirs,

I showed him by the poor-house returns, that the number of paupers off his estate and receiving relief in the workhouse amounted to about three thousand. That I was wholly unable to undertake the employment of these people in their present condition, on reproductive works; and that if left in the workhouse, the smallest amount they could possibly cost would be £5 per head per annum, and thus that the poor rates must necessarily amount, for some years to come, to £15,000 per annum, unless these people died or left—and the latter was not probable . . . I explained to him further, that . . . inasmuch as the poor rates were a charge prior to the rent, it would be impossible for his lordship to expect any rent whatever out of his estate for many years to come. The remedy I proposed was as follows. That he should forthwith offer *free emigration* to every man, woman, and child now in the poor-house and *chargeable to his estate* . . . That even supposing they all accepted this offer, the total, together with a small sum per head for outfit and a few shillings on landing, would not exceed from £13,000 to £14,000, a sum less than it would cost to support them in the workhouse for a single year . . . I plainly proved that it would be cheaper to him, and *better for them*, to pay for their emigration at once, than to continue to support them at home.

Lansdowne must have been predisposed to accept Trench's reasoning, writing him a check on the spot for £8,000 (equivalent to about \$850,000 today) to be used to initiate the project. By the end of 1851, Lansdowne had spent £9,500 (slightly more than \$1 million today) on emigration.³²

Trench later recalled that when he returned to the estate and announced the emigration offer, "it was considered by the paupers to be too good news to be true. But when it began to be believed and appreciated, . . . they rushed from the country like a panic-stricken throng, each only fearing that the funds at my disposal might fail before he and his family could get their passage." Each week, Trench wrote, he chose from the poorhouse population 200 "of those apparently most suited for emigration; and having arranged their slender outfit," put them in the hands of an employee who led them on the sixty-mile journey to Cork, with orders "not to leave them nor allow them to scatter, until he saw them safely on board the emigrant ship . . . Week after week, to the astonishment of the good people of Cork, and sometimes not a little to their dismay, a batch of two hundred paupers appeared on the quays of Cork, bound for the Far West . . . And thus, two hundred after two hundred, week after week, departed for Cork, until the poor-house was nearly emptied of paupers chargeable to the Lansdowne estate; and in little more than a year 3,500 paupers had left Kenmare for America, all free emigrants, without . . .

³² Trench, *Realities of Irish Life*, 122–24. Lansdowne had previously expressed fears that poor relief taxes would ruin those in his position. When Parliament enacted legislation in 1849 forcing Irish landlords to pay an even larger proportion of famine relief, Lansdowne had condemned it as "nothing less than a scheme of confiscation, by which the weak would not be saved, but the strong be involved in general ruin." Cecil Woodham-Smith, *The Great Hunger: Ireland, 1845–1849* (1962; rpt. edn., New York, 1991), 379.

the slightest pressure put upon them to go.” Only fifty or so of the Lansdowne tenants in the workhouse chose to forgo the emigration offer.³³

TRENCH BOASTED IN HIS MEMOIRS that he allowed each emigrant “to select what port in America he pleased—whether Boston, New York, New Orleans, or Quebec.” Nearly all chose New York. Estate records indicate that 1,700 Lansdowne tenants left Kenmare for New York from December 1850 through March 1851. From that point, perhaps in an effort to save money, Trench no longer offered the emigrants their choice of destination, instead requiring them all to sail to Quebec. By the end of 1851, another 1,300 had departed for that port, although most of these emigrants eventually made their way to Manhattan as well.³⁴

Even in a huge metropolis such as New York, where more than 250,000 immigrants landed in 1851, the unprecedented poverty and raggedness of Lansdowne’s former tenants evoked an immediate public reaction. On March 19, the *New York Tribune* reported that “several destitute emigrants who arrived in this city a few days ago by the ship *Montezuma* [Ellen Holland’s vessel], from Liverpool, were found Monday afternoon in the streets, in a starving condition. They were taken to the Fourth Ward [Police] Station, where they were provided with food, after which they were sent to the Commissioners of Emigration,” who were responsible for assisting indigent immigrants. “These emigrants, it appears, were taken out of the poorhouses in Ireland, by Lord Lansdowne.” A few days after the *Tribune* story appeared, the *Sir Robert Peel* arrived in New York Harbor from London. The Lansdowne emigrants disembarking from that ship presented such a spectacle of wretchedness that it prompted an entire editorial in the *Herald*:

IRISH EMIGRANTS.—It is really lamentable to see the vast number of unfortunate creatures that are almost daily cast on our shores, penniless and without physical energy to earn a day’s living. Yesterday, groups of these hapless beings were to be seen congregated about the [City Hall] Park and in Broadway, looking the very picture of despair, misery, disease and want. On enquiry, we ascertained that they had arrived here by the ship *Sir Robert Peel*, and that they had been, for the most part, tenants of the Marquis of Lansdowne, on his county Kerry estate—ejected without mercy by him, and “shipped” for America in this wholesale way. Among them were gray haired and aged men and women, who had spent the heyday of their life as tillers of their native soil, and are now sent to this country to find a grave. This is too bad—it is inhuman; and yet it is an act of indiscriminate and wholesale expatriation committed by the “liberal” President of the Council of her Majesty Queen Victoria’s “liberal” ministry.

In the space of only a few days, the two most influential newspapers in the United States had taken the highly unusual step of singling out the Lansdowne immigrants

³³ Trench, *Realities of Irish Life*, 124–25.

³⁴ Lansdowne estate records in Lyne, “Post-Famine Emigration from Kenmare,” 136–37. For some of the Lansdowne ships, see Glazier, *Famine Immigrants*, 6: 619–20, 626–27, 662–63, 644–49, 7: 16–19, 84–85. Although most of the Lansdowne Papers were recently sold to the British Library, the papers relating to his Irish estate are still in the possession of his descendants, who now charge £100 per day to anyone wishing to examine them. Before these fees were put in place, Irish historian Gerard Lyne was given free access to these papers, and I decided to rely on his very thorough analysis of those records in this and subsequent paragraphs.

TABLE 1
Age Distribution of Irish Immigrants Arriving in New York in 1851

| | <i>Age 16 or younger</i> | <i>Age 17 to 35</i> | <i>Age 36 or older</i> |
|-------------------------|--------------------------|---------------------|------------------------|
| Lansdowne Immigrants | 43% | 37% | 20% |
| Random Irish Immigrants | 22% | 74% | 4% |

SOURCE: See n. 36.

for comment and condemnation, indicating that their condition upon arrival must have been extraordinarily bad. Even in a city teeming with impoverished newcomers, the plight of the Lansdowne emigrants aroused special indignation.³⁵

The charge that the Lansdowne immigrants were disproportionately “gray haired and aged” deserves attention. At first glance, the charge seems unfounded, as the average age of the Lansdowne immigrants—22.1 years—was slightly *lower* than that of the average Irish immigrant (22.3 years) arriving in New York at that time. But an analysis of the age distribution of the assisted emigrants bears out the Americans’ complaints. The average Irish immigrant was twice as likely to be in his or her prime work years—ages seventeen to thirty-five—as was a Lansdowne immigrant. Although the majority were far from “gray haired and aged,” the Lansdowne immigrants were four times more likely to have surpassed their fiftieth birthday than the typical Irish immigrant. Assisted emigrants also brought with them many more dependents (young children and aged parents) than did the average Irish immigrant. On the *American Eagle*, a ship probably made up entirely of Lansdowne tenants, 98 percent of the emigrants traveled with another family member. In contrast, only 55 percent of Irish immigrants traveled with one or more family members on the typical immigrant ship arriving in New York in this period. Because the marquis paid for entire families to emigrate, women were unusually numerous on the Lansdowne vessels, composing 49 percent of the adults versus only 31 percent on the average Irish immigrant ship. Thus, while the Lansdowne immigrants were hardly all superannuated, many fewer of them were the vigorous young men Americans perceived to be the ideal, self-supporting immigrant.³⁶

By April, the Irish press had reprinted the New York news stories concerning the Lansdowne emigrants, condemning the marquis and Trench for mistreating their helpless tenants. Trench defended himself, insisting in a letter that many of the emigrants supplied with clothes had asked to keep their old rags. “They wanted them as convenient garments to put on and beg with in New York,” Trench insisted, a charge he repeated in his memoirs more than fifteen years later.³⁷

³⁵ *New York Tribune*, March 19, 1851; *New York Herald*, March 22 (the *Sir Robert Peel*’s arrival), 23 (editorial), 1851.

³⁶ Lansdowne sample based on entire passenger list of the *American Eagle*. “Random Irish” based on a random sample of ships that arrived in New York from the same ports as the Lansdowne vessels (London and Liverpool) and in the same months of 1851. See Glazier, *Famine Immigrants*, 6: 435, 615–18, 652–56, 662–63, 7: 34–35.

³⁷ Trench to Henry A. Herbert, [April 1851], Bowood Papers, Bowood House, quoted in Lyne, “Post-Famine Emigration from Kenmare,” 111; Trench, *Realities of Irish Life*, 125–26.

Even if some immigrants did hide their new clothes in order to better plead for assistance, Trench himself deserves full blame for putting them in a position to have to ask for handouts at all. Estate records show that of the 1,700 emigrants sent to New York in 1851, Trench supplied clothing to at most 226. In addition, 1,350 of Lansdowne's New York-bound emigrants received no food whatsoever for the voyage. They were, in the words of one Irish journal, "obliged to subsist on the ship's allowance—an allowance which is scarcely sufficient to keep a full grown person from starvation. God help them!" This was no exaggeration, as the "ship's ration" consisted each day of just one pound of bread, meal, biscuits, or flour and only thirteen ounces of water. Each of Palmerston's assisted emigrants, in contrast, received each week, in addition to the ship's allowance, six pounds of biscuits, three-and-a-half pounds of flour, one pound of pork or beef, one pound of sugar, one pound of rice, eight ounces of treacle, four ounces of coffee, and two ounces of tea. To the charge that he had not properly supplied the emigrants, Trench admitted privately that "this is to a certain extent true but it would have cost thousands more to do it otherwise." Trench likewise justified his failure to give each emigrant the promised "few shillings" each to help with settlement in America on the grounds that most already owed the estate two to four years' back rent. Despite his public insistence that he had done nothing wrong, Trench began spending significantly more outfitting each emigrant after the press complained about his parsimony. Beginning in mid-April, when the *Tribune* and *Herald* reports reached Ireland, spending on supplies per emigrant increased more than twenty-fold, from about 9 pence (about \$4 today) per emigrant to about 17 shillings (\$90 today). Despite the increased outlay on supplies, Lansdowne quickly recouped his expenditures in reduced poor taxes. The Kenmare workhouse, which in 1850 had housed 2,500 Tuosist and Bonane paupers chargeable to Lansdowne, contained by 1853 only fourteen inhabitants from those parishes.³⁸

Trench argued that his employer was not the only one to benefit from the emigration program. "The most cheering accounts are daily reaching us of their success in New York," Trench informed Lansdowne in mid-April, by which point news from none but the very first emigrants could have reached Kenmare. "Considerable sums of money have been already sent over in very small remittances of 20s[hillings] or 30s each, and every letter which arrives brings new accounts of how well they fare and urging others to come over if they can." Two years later, Trench sounded the same refrain, bragging that "large sums are . . . coming over from America." Later, he reported that some emigrants had returned to Kerry sporting gold chains. "Others are receiving money to pay their rents," Trench insisted, "and we are certainly receiving back *good interest at least* for the money expended on emigration." Whether or not the Lansdowne emigrants wrote glowing accounts of their new homes cannot be verified, as none of their correspondence is

³⁸ "Return of Emigration off the Estate of the Marquis of Lansdowne from 1 Dec. 1850 to 1 Feb. 1852," in 1852 Trench annual report, Derrreen Estate Papers; *Waterford Chronicle* in *Kerry Evening Post*, April 26, 1851; Trench to Herbert, April 18, 1851; Trench to Lansdowne, September 16, 1853, Bowood Papers, all in Lyne, "Post-Famine Emigration from Kenmare," 89, 104, 110, 112–13, 136–37. The food Palmerston supplied to his emigrants is described in S. Maxwell to Messrs. Stewart and Kincaid, November 27, 1847, in *Irish University Press Series of British Parliamentary Papers*, "Colonies, Canada" series (Shannon, 1969), 17: 353–54.

known to have survived. Father O'Sullivan later complained that Trench should have been "ashamed" at the way he exaggerated the success of "the victims of your ill-advised extermination." On the other hand, it would have been wholly in keeping with the tenor of emigrants' typically boastful letters for them to write, as Trench claimed, that they were "now living as well as Father McCarthy himself."³⁹

THANKS TO A NUMBER OF FORTUITOUS CIRCUMSTANCES, we can reconstruct the New York lives of the Lansdowne immigrants with a degree of precision and detail unprecedented in the literature on Irish famine-era migration. Because the secretary of Five Points' Roman Catholic church recorded unusually detailed biographical information in the parish's marriage register, one can identify the concentration of Lansdowne immigrants in the neighborhood and pinpoint their residential patterns to a remarkable extent. The recent opening of records from the Emigrant Savings Bank allows the historian to recreate the Lansdowne immigrants' occupational and financial status with equal accuracy. And because these immigrants chose to settle in such a notorious district, descriptions of their tenements abound as well.

Why the Lansdowne emigrants chose to concentrate in Five Points is not known. No longstanding Kerry enclave attracted these immigrants to the neighborhood. Kerry natives may have chosen to live there because, as the most destitute of immigrants (even by Irish famine standards), they gravitated to the cheapest housing in the city, much of which was in Five Points. Whatever their impetus for settling there, by 1860 about one in seven Five Points Irish Catholics was a Kerry native. More than 75 percent of these Kerry immigrants had once lived on the Lansdowne estate. Given that about two-thirds of the neighborhood's 14,000 residents in 1855 were Irish natives or their children, one can estimate that roughly 1,000 Five Points inhabitants were former Lansdowne tenants or their offspring. This sizable population resulted not merely from the massive emigration program of 1851 but also from the subsequent immigration underwritten by friends and relatives in New York, as well as occasional passages financed by Lansdowne after 1851. Of the 1,000 or so Lansdowne immigrants living in Five Points in 1855, approximately 500 had arrived in New York as part of the main Lansdowne flotilla in early 1851, while most of the remainder had emigrated later.⁴⁰

Neither the first Lansdowne immigrants nor those who joined them later chose haphazardly where within Five Points to reside. Each of the dominant Irish sub-

³⁹ Trench to Lansdowne, April 15, 1851, September 16, 1853, Bowood Papers; O'Sullivan to Trench, February 12, 1869, in *Tralee Chronicle*, February 16, 1869; Trench annual report of 1851, p. 6, Derreen Estate Papers, all in Lyne, "Post-Famine Emigration from Kenmare," 92–94; Trench, *Realities of Irish Life*, 126–27.

⁴⁰ For the source of my figures on Kerry natives in Five Points, see n. 16 above. My estimate that 500 of the Five Points Lansdowne immigrants came with the bulk of the assisted emigrants in early 1851 is based on the Emigrant Savings Bank records. Of the 81 former Lansdowne tenants who opened bank accounts by mid-1855 when the census was taken, 52 (64 percent) had arrived in New York by May 1851 when the last of the 1,700 emigrants sent directly to New York arrived there. Yet many post-1851 immigrants did not open accounts until after mid-1855, so I have estimated that about half the Lansdowne Five Points population in 1855 had arrived with the main flotilla. Accounts 1–10,000, Emigrant Savings Bank Test Books.

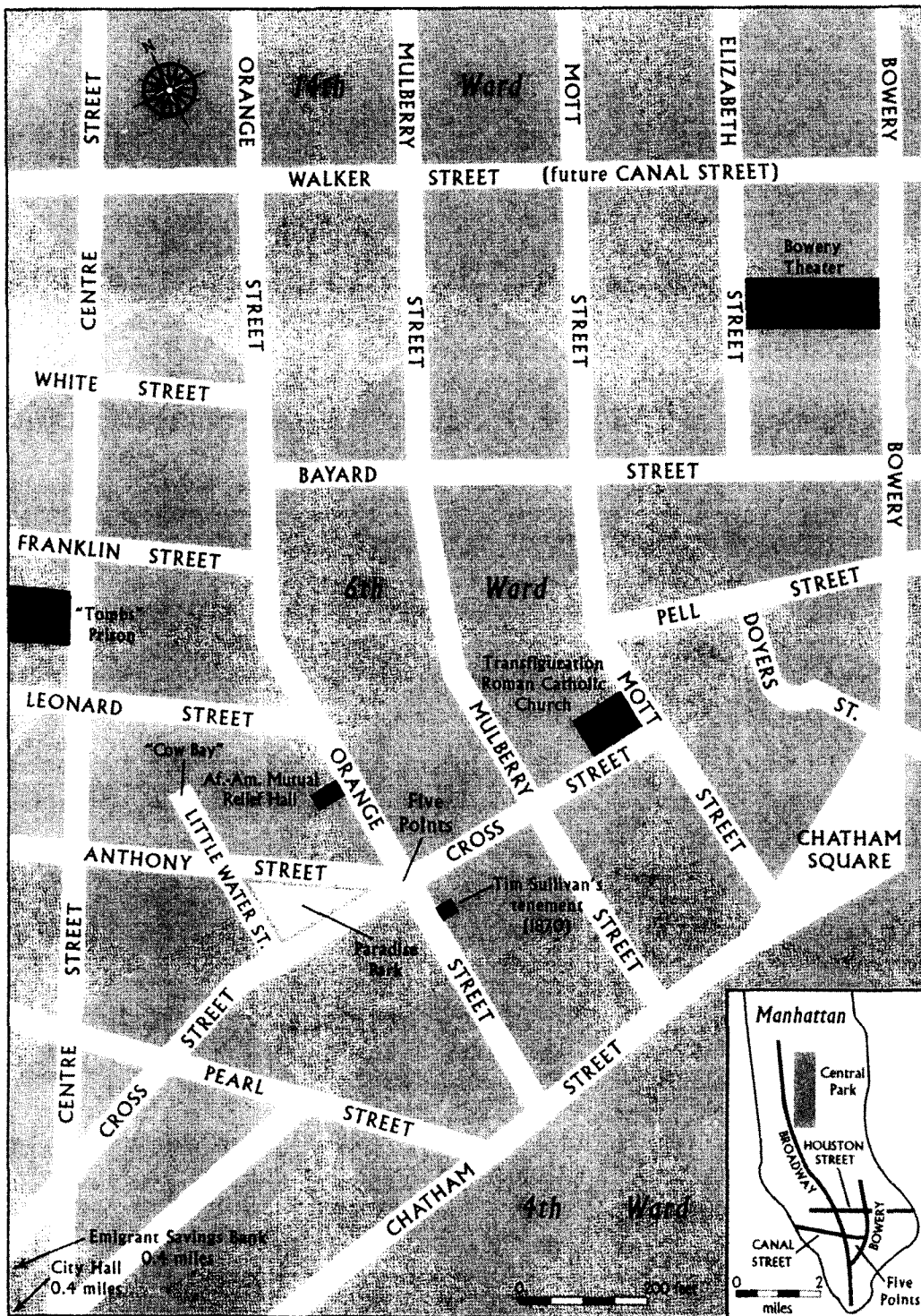


FIGURE 2: Map of the Five Points neighborhood in Manhattan, 1851. Mapmaker Chris Robinson.

groups in the neighborhood—those from counties Sligo, Cork, and Kerry—created enclaves to some degree. But the Kerry immigrants were by far the most clannish of the three, with 84 percent of them crowded into just two of the neighborhood's twenty or so blocks. These two blocks, Orange Street from Anthony to Leonard and Anthony Street from Centre to Orange, were two of the five blocks whose confluence gave Five Points its name.⁴¹ Kerry immigrants dominated those blocks, which were among the most notoriously squalid and crime-ridden in Five Points and all New York. Sixty-four percent of the Irish Catholic residents identified on those two blocks were Kerry natives. Seventy-nine percent of those Kerry natives had emigrated from the Lansdowne estate. Lansdowne immigrants often took over whole buildings within this zone. Eighty-six percent of the Irish Americans in the five-story building at 31 Orange Street, for example, had emigrated from Kerry. Of those Kerry natives, 88 percent were Lansdowne emigrants. Sligo and Cork natives never concentrated together to the degree that the Lansdowne immigrants did in their enclave.⁴²

We know from various newspaper exposés and legislative investigations that the tenements in which the Lansdowne immigrants clustered were Five Points' very worst. The northernmost address in the Lansdowne enclave, for instance, was 39 Orange Street. Less than two years before the Lansdowne immigrants arrived, 106 hogs had been kept at this address along with the human inhabitants. In 1856, investigators sent by the state legislature found no more swine, but they were still astounded by the filth and crowding. As was the case at most of the addresses in this part of Five Points, each lot held both a front and rear tenement. Rear tenements were the most notorious abodes in the neighborhood. These small, ramshackle buildings were usually trapped in perpetual shadow as the larger surrounding structures blocked virtually all direct sunlight. In most cases, the only windows on rear tenement buildings faced the noxious outhouses in the yard between the front and rear structures, meaning that the stench from the privies permeated the rear buildings.

At 39 Orange, the alley leading from the street to the rear tenement measured only nineteen inches wide in some places, and only two feet at its widest. Squeezing through to the rear building, the legislative committee discovered fifteen people living in a single room measuring fifteen by fourteen feet. This was probably the household of Tuosist native Barbara Sullivan. A year earlier, the 1855 state census enumerator had found the widowed fifty-year-old living there with her six children (ranging in age from four to sixteen) and six lodgers. Sullivan had arrived in New York in September 1851 just after the main Lansdowne flotilla. At the time of the census taker's visit, Sullivan lodged a forty-year-old widowed rag picker and her fifteen-year-old newsboy son, as well as a forty-year-old widowed "hawker" and her three children, the eldest of whom worked as a household servant. Ellen Holland

⁴¹ At the end of 1854, Orange Street was renamed Baxter, and Anthony Street became Worth. To avoid confusion, I have referred to them by their original names throughout this article, even though some of the quotations below are from the period after the names had changed.

⁴² Some 119 of the 185 residents identified on these blocks in the Transfiguration marriage records were natives of Kerry. Of those 119, 94 had been born on the Lansdowne estate. Note that at 31 Orange Street, Irish families occupied only 63 percent of the apartments according to the 1855 state census. The remaining residents were mostly Italians.

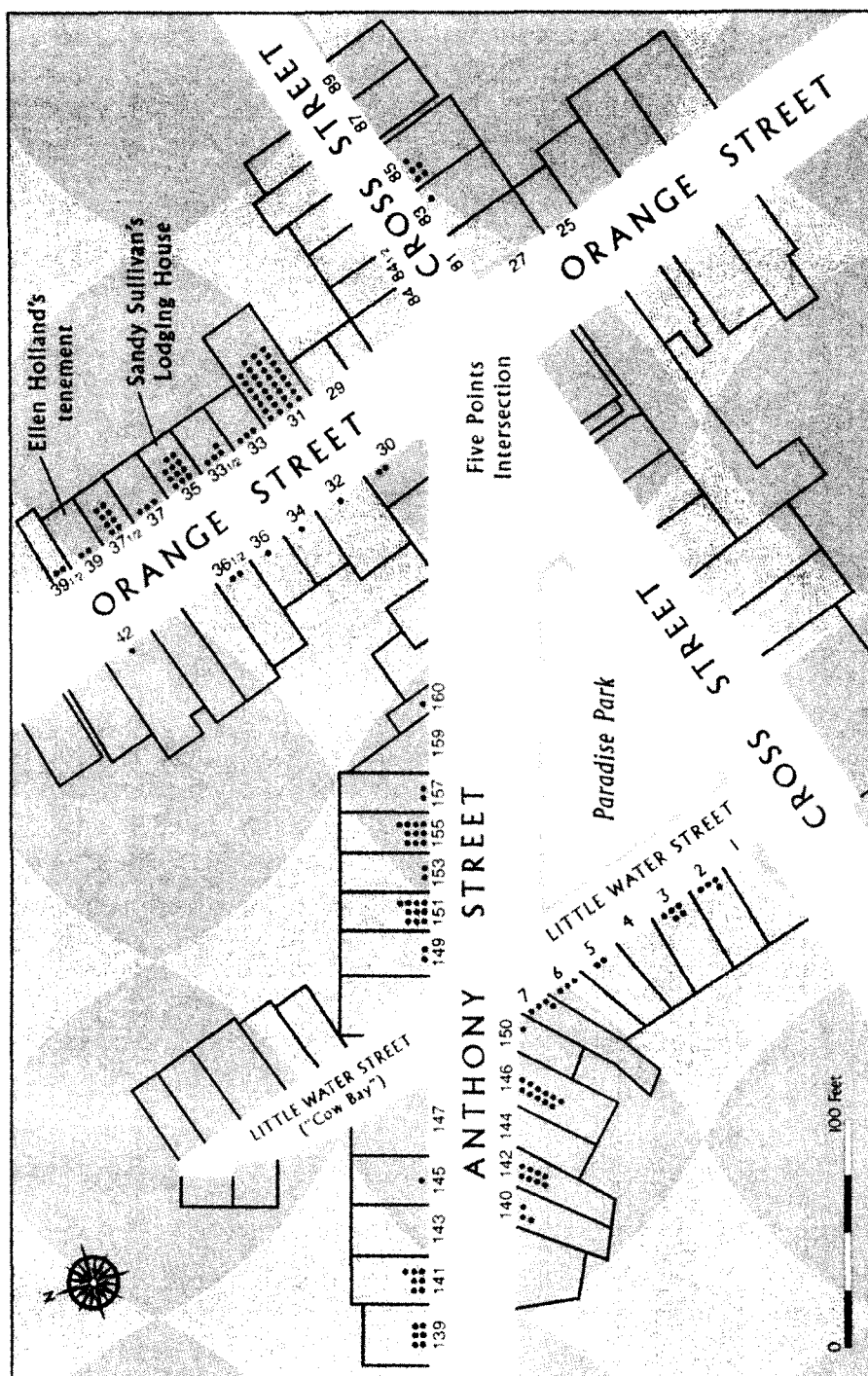


FIGURE 3: Map of Lansdowne enclave within Five Points, showing concentration of Lansdowne immigrants by building. Mapmaker Chris Robinson. Each dot represents one Lansdowne immigrant who was either married at the Church of the Transfiguration from 1853 to 1860 or who opened a bank account at the Emigrant Savings Bank from June 1851 to August 1856. Transfiguration Church Marriage Register, 29 Mott Street, New York; Emigrant Savings Bank Collection, Test Books and Account Ledgers, New York Public Library.

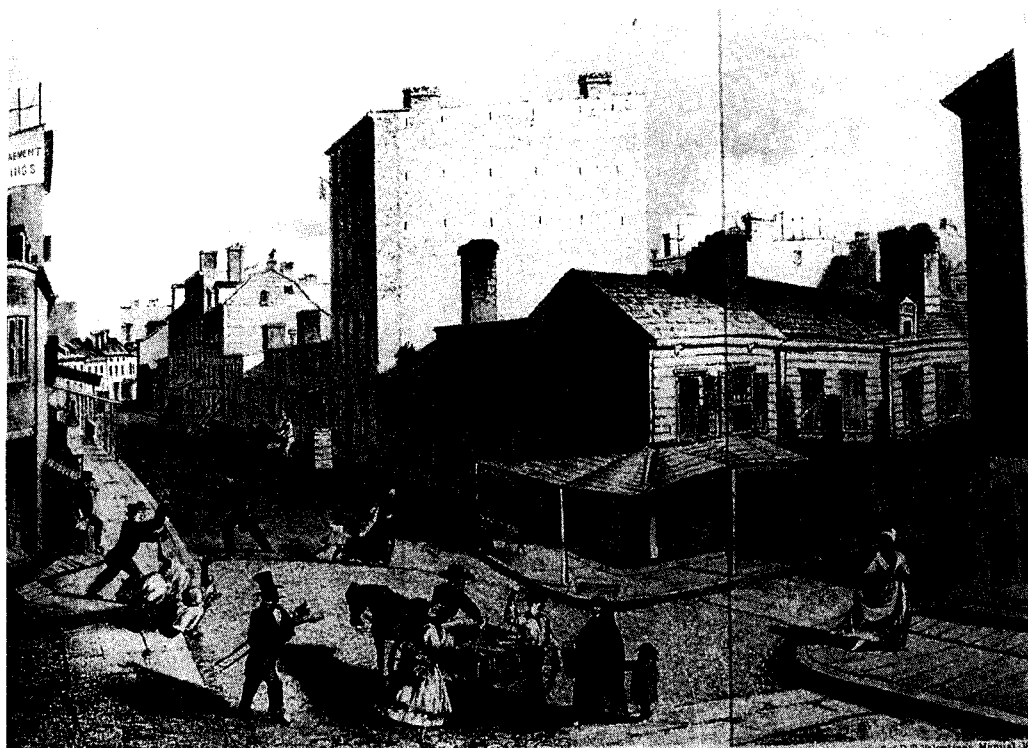


FIGURE 4: Lithograph showing the Five Points intersection, 1860. The left side of this print depicts one of the blocks on which the Lansdowne immigrants concentrated, the stretch of Orange Street (later renamed Baxter) that ran north from the Five Points intersection. The large five-story building at the center is 31 Orange Street, whose three-room apartments housed dozens of Lansdowne immigrants during the 1850s. The small run-down buildings to the left, which also housed many Lansdowne immigrants, were the most notorious on the block. D. T. Valentine, ed., *Manual of the Corporation of the City of New York for 1860* (New York, 1860).

also lived at this address from at least 1853 through 1855, though whether in the front or rear tenement is not known. While the legislators made no other comments about this residence, its inclusion in their report indicates that it must have been one of the worst in this miserable district.⁴³

As one moved south from 39 Orange toward the Five Points intersection, the frightful living conditions continued. Next door, 37 ½ Orange was also dominated by Lansdowne immigrants. The legislative inspectors found that as in most other parts of the neighborhood, the vast majority of the inhabitants spent the night in “sleeping closets,” tiny windowless bedrooms that rarely measured more than six by eight feet. Among the residents at this address, one widowed “old dame of sixty” and her two daughters “supported themselves by picking curled hair sixteen hours a day, the three earning five dollars per week.” The “old dame” was probably Honora Moriarty, who the 1855 census taker had found at this address along with twenty-year-old Margaret and sixteen-year-old Mary Moriarty. These three women and a fourth lodger shared an apartment with Denis Shea, his two young children, and his wife Mary and her brother Michael. Mary and Michael were themselves

⁴³ *New York Evening Post*, May 17, 1849; “Report of the Select Committee Appointed to Examine into the Condition of Tenant Houses in New-York and Brooklyn,” *Documents of the Assembly of the State of New-York, 80th Session—1857* (Albany, N.Y., 1857), doc. 205, p. 18.

Moriartys, though not directly related to the boarders. Such arrangements indicated the continuing strength of “clanship” noted by Tuosist’s Father McCarthy.⁴⁴ Lansdowne immigrants took in far more boarders than the typical Five Points family. The majority of Five Points households did not take in any lodgers at all, and those that did typically took in only one or two. The Lansdowne immigrants’ propensity to take in large numbers of boarders in 1855, four years after most had arrived in the United States, indicates that they were still anxious to supplement their work-related income.⁴⁵

While most Lansdowne immigrants rented space in their own homes to a few lodgers, some became full-time boardinghouse keepers, outfitting a basement or series of apartments with bunks to accommodate as many lodgers as possible. Such was the case at 35 Orange Street, another Lansdowne-dominated building, whose basement boarding establishment was referred to sarcastically by the *New York Illustrated News* as “Mrs. Sandy Sullivan’s Genteel Lodging-House,” operated by Lansdowne immigrants Sandy and Kate Sullivan from Tuosist. The Sullivans had arrived in New York with the main Lansdowne flotilla in March 1851. A *Times* reporter, stopping at their boardinghouse during a tour of Five Points in 1859, called it “one of the filthiest, blackest holes we had yet seen.” The proprietor of this “damp and filthy cellar . . . with much loquacity, assured [us] that the bed-clothes were all ‘clane and dacent sure,’ that they were washed ‘onst a week,’ every Thursday, and that the place was quite sweet.” Around the main room, the reporters saw

a number of wretched bunks, similar to those on shipboard, only not half as convenient, ranged around an apartment about ten feet square. Nearly every one of the half-dozen beds was occupied by one or more persons. No regard was paid to age or sex; but man, woman, and child were huddled up in one undistinguishable mass . . . The most fetid odors were emitted, and the floor and the walls were damp with pestiferous exhalations. But this was not all. There were two inner apartments, each of which was crowded to the same capacity as the outer one. Not the slightest breath of air reached these infernal holes, which were absolutely stifling with heat.

Inquiring about two small children sleeping soundly in one of the “hideous beds,” the manager told the reporters that their older sister, who cared for them, “was out begging, even at this hour.” Hard as it may be to believe, this lodging house must have been superior to many others in the neighborhood—to stay there cost six cents a night, far more than the worst dives. Sandy Sullivan told the *Times* that he “lodged none under any circumstances but honest hardworking people—which statement the police received with smiles and without contradiction.” “To do them justice,”

⁴⁴ I cannot document that these two families were former Lansdowne tenants, but their surnames and residence in a building particularly dominated by such tenants makes it likely that my identification is accurate.

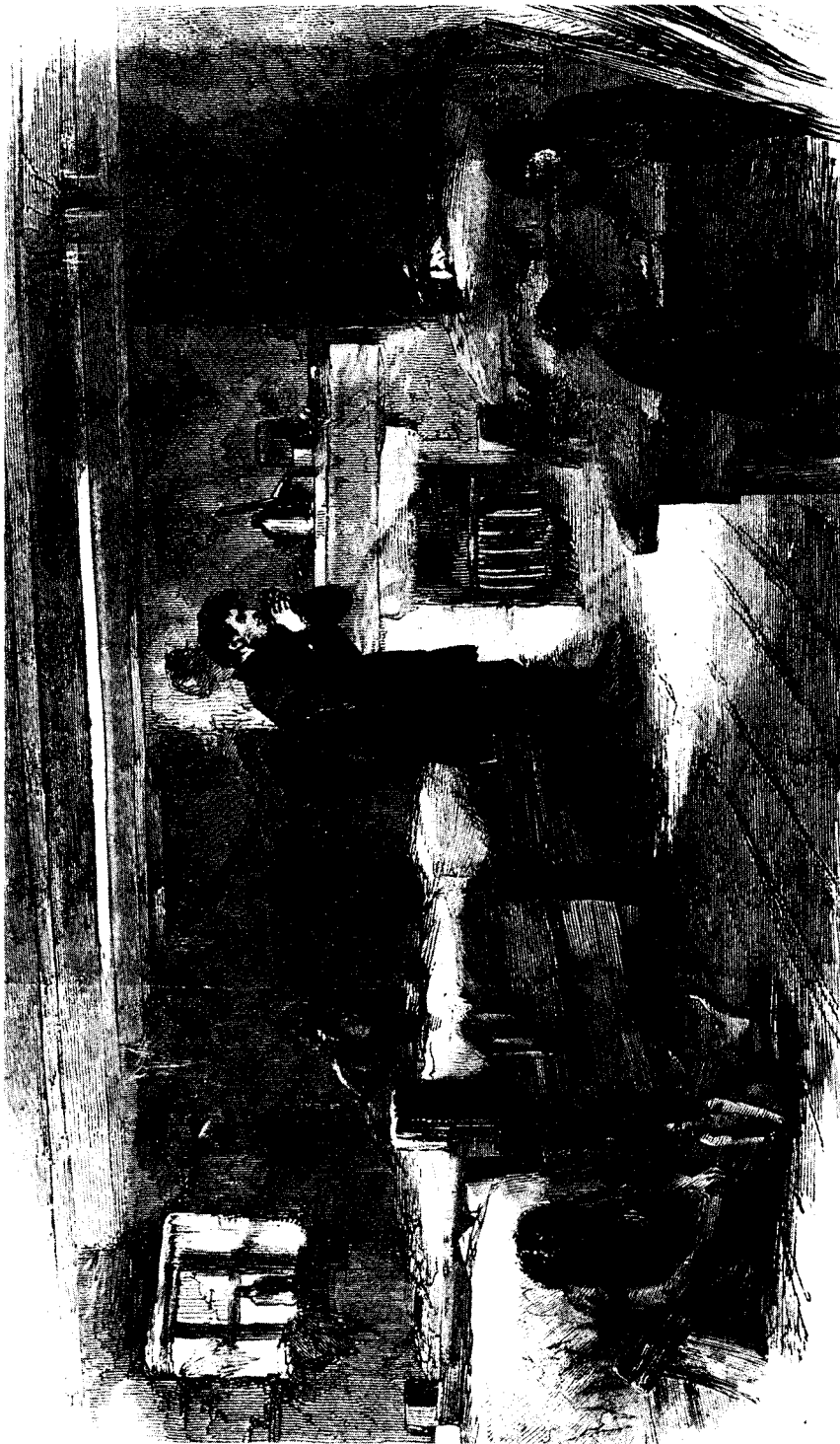
⁴⁵ Twenty-eight percent of Five Points families rented space to boarders. Two-thirds of these families took in only one or two non-family members. These figures based on my analysis of Districts Three, Four, Five, and Six, Sixth Ward, 1855 New York State manuscript census returns, Old Records Division, New York County Clerk’s Office. (I consulted a microfilm copy made by the Church of Jesus Christ of Latter-day Saints Family History Library.)

agreed an *Express* reporter accompanying the journalist from the *Times*, “such as were awake seemed to be quite sober.”⁴⁶

Continuing southward from the Sullivans’ lodging house toward the Five Points intersection, one reached 31 Orange Street. The five-story brick tenement at this address, one of the first residential structures of its size in the neighborhood, towered over the wooden hovels just described. Conditions in this building did not attract the attention of investigators in the antebellum period, perhaps because as a relatively new building it had not had much time to deteriorate. Yet the terrible crowding in its three-room (325-square-foot) apartments boggles the mind. Cornelius Shea and his wife Ellen, Lansdowne emigrants from Kenmare parish who had arrived in New York in April 1851, shared their apartment with three children and four lodgers. Widow Johanna McCarty squeezed her four children (ranging in age from six to twenty) and eight lodgers into her three-room flat. Three other dwellings held nine persons each, including that of Tuosist native Daniel Haley, another 1851 Lansdowne emigrant. Poverty did not push all Kerry natives to take in so many lodgers. Mary Shea of Kenmare and her husband Jeremiah, a Cork native, who had immigrated together in 1852, lived at 31 Orange with their twenty-three-year-old daughter Margaret (a servant) and just a single lodger. The combined incomes of father and daughter, plus the profit from the lodger, allowed them to forgo the intense crowding endured by their neighbors. Laborer Daniel Hagerty, another 1851 Lansdowne emigrant from Kenmare, also lived at 31 Orange in 1855 with his wife Mary and his younger brother Patrick, also a laborer. Their incomes enabled them to dispense with boarders altogether. Thus, while the relatively large apartments at 31 Orange (two-room, 225-square-foot abodes were more typical) provided more spacious accommodations for families with dual incomes, their size merely encouraged many renters to cram even more lodgers than usual into the still crowded spaces.⁴⁷

⁴⁶ *New York Times*, July 1, 1859; *New York Express* quoted in Samuel B. Halliday, *The Lost and Found: or, Life among the Poor* (New York, 1859), 207–08; *New York Illustrated News* (February 18, 1860): 216. I have identified this boardinghouse as the one portrayed in the *Illustrated News* because the only “Sandy” Sullivan I came across out of the hundred or so Sullivans in the various Five Points records is the Sandy Sullivan listed as living at 35 Orange by his daughter Mary when she opened a savings account in March 1854. See Account 6524, Emigrant Savings Bank Test Books; Glazier, *Famine Immigrants*, 6: 647. Sandy and Catharine Sullivan (both age 45), with their four children (aged 4 to 22) and eight boarders, are listed in the 1860 census, apparently at that same address. See Family 227, p. 136, Second District, Sixth Ward, 1860 United States manuscript census, National Archives.

⁴⁷ Dwelling 31, Second Division, Third Electoral District, Sixth Ward, 1855 New York Census; Accounts 4983, 5800, 6773, and 7504, Emigrant Savings Bank Test Books; wedding of Daniel Haley, October 14, 1857, Marriage Register, Church of the Transfiguration, 29 Mott Street, New York. The other block dominated by Lansdowne immigrants, Anthony Street from the Five Points intersection to Centre Street, was virtually as squalid as Orange Street. Its most notorious stretch, a cul-de-sac jutting off to the north known officially as Little Water Street but popularly referred to as Cow Bay, was renowned for “the extreme wretchedness which abounds on every hand.” Experts called Cow Bay “the very lowest and worst place in New-York.” By 1857, New Yorkers associated Cow Bay with Kerry immigrants, but because of the imprecise numbering of its houses, it is impossible to determine with certainty if any Lansdowne immigrants lived there. *The Old Brewery and the New Mission House at the Five Points, by the Ladies of the Mission* (New York, 1854), 104 (“extreme wretchedness”); *Monthly Record of the Five Points House of Industry* 1 (January 1858): 219 (“very lowest”); Solon Robinson, *Hot Corn: Life Scenes in New York Illustrated* (New York, 1854), 209, 307; “EYE WITNESS” to the Editor, *New York Times*, July 7, 1857. Overcrowding was a problem for the Irish who settled in England as well. See John Haslett and W. J. Lowe, “Household Structure and Overcrowding among the Lancashire Irish, 1851–1871,” *Histoire sociale* 10 (May 1977): 45–58; and Enid Gaudie, *Cruel Habitations: A History of Working-Class Housing, 1780–1918* (London, 1974).



BACKGROUNDS OF CIVILIZATION.—MRS. SANDY SULLIVAN'S GENTLE LODGING-HOUSE IN BAXTER STREET.—See page 209.

FIGURE 5: Woodcut of "Sandy Sullivan's Lodging House," 1860. Probably the establishment toured by the *New York Times* and *New York Express* reporters in 1859. Most Lansdowne immigrants took in borders informally to supplement their incomes, but some, like Sullivan, eventually expanded the practice into a full-time occupation. *New York Illustrated News* (February 18, 1860): 216.

Genteel Americans wondered how and why immigrants tolerated such miserable conditions. Compared to their Irish cabins, however, Five Pointers may have perceived their tenements, at least initially, as somewhat pleasant. Five Pointers had wooden instead of dirt floors, typically with “bits of carpits” on them. Immigrants also undoubtedly preferred their plaster ceilings to the insect-filled thatch roofs that they had known in Kerry, and in winter a tenement’s warm stove was far preferable to the open fire typically found in smoke-filled Irish cabins. Two large windows per dwelling did not seem like many to American natives, but to an Irish immigrant whose home had never before held more than a tiny pane or two, Five Points apartments must have seemed almost bright. And given that they had generally lived in Ireland in a single ten-by-thirteen-foot room, the typical two-room, 225-square-foot Five Points apartment might have seemed spacious, at least to those who did not take in many boarders. So while conditions in the Lansdowne immigrants’ tenements were bad, they were really no worse, and in some ways were actually much better, than their living conditions in Ireland. This undoubtedly explains why even Five Points immigrants could write glowing, boastful letters to loved ones back in Ireland.⁴⁸

ONE WOULD IMAGINE that Lansdowne immigrants chose to live in such dilapidated, overcrowded tenements because they could afford nothing else. In fact, male Lansdowne immigrants did hold the city’s lowest-paying, least desirable jobs. Lansdowne immigrants were much more likely than the typical Irish Five Pointer to toil as a menial day laborer, the job held by the overwhelming majority of the neighborhood’s unskilled workers (see Table 2). Conversely, natives of the Lansdowne estate were significantly less likely to own their own businesses or hold lower-status “white-collar” jobs and were ten times less likely than the typical Five Pointers to work as skilled artisans.⁴⁹

Frustratingly little is known about the lives of nineteenth-century America’s day laborers. Construction jobs probably provided most of such workers’ employment. Day laborers could dig foundations, carry heavy hods full of bricks and mortar to

⁴⁸ [James D. Burn], *Three Years among the Working-Classes in the United States during the War* (London, 1865), 14–15 (quotation). Although few letters written by Five Points immigrants survive, see “Eliza Quin” to “Dear Parents,” January 22, 1848, in House of Commons, “Colonisation from Ireland: Select Committee, House of Commons, Third Report with Appendix,” *Sessional Papers*, 1849 (86), 11: 128, in *Irish University Press Series of British Parliamentary Papers*, “Emigration” series (Shannon, 1969), 5: 128.

⁴⁹ For the occupational comparisons in this paragraph and in Table 2, I grouped Five Pointers’ vocations into the following categories: The single Irish-born professional in my Five Points sample was a physician. “Business owners” include shopkeepers, grocers, and food and liquor dealers. It is impossible, through the census, to determine if a “grocer” actually owned his or her own business or merely worked as a clerk for someone else. For the sake of consistency, all grocers have been placed in the “business owners” category. Lower-status white-collar workers were overwhelmingly clerks, but this category also includes a few non-clerical government employees who were neither skilled craftsmen nor menial laborers. The skilled manual workers category is composed of bedstead makers, blacksmiths, brass workers, bricklayers, cabinetmakers, cap makers, carpenters, carpet weavers, confectioners, coopers, hatters, locksmiths, machinists, masons, millers, musicians, plasterers, painters, plumbers, printers, shoemakers, silver and tin smiths, tailors, umbrella makers, and varnishers. Unskilled workers include laborers, cartmen, expressmen, porters, sailors, hostlers and grooms, waiters, watchmen, and policemen (so classified because people from any occupational category took these jobs when offered). Finally, “difficult to classify” includes occupations such as gentleman, speculator, “flayer,” farmer, and a “pocket book manufactory” worker.

TABLE 2
Irish Men's Employment by Occupational Category, 1850–1855

| | <i>Lansdowne Five Points Bank Depositors</i> | <i>All Irish-Born Five Points Bank Depositors</i> | <i>All Irish-Born Five Points Residents</i> |
|-----------------------------|--|---|---|
| Professionals | 0% | 0% | .4% |
| Business Owners | 0% | 6% | 3% |
| Peddlers and Street Vendors | 5% | 10% | 1% |
| Lower-Status White-Collar | 0% | 9% | 3% |
| Skilled Manual Workers | 3% | 30% | 34% |
| Unskilled Workers | 90% | 43% | 58% |
| Difficult to Classify | 2% | 1% | 1% |

NOTES: All percentages rounded to nearest whole number except those smaller than one, which are rounded to nearest tenth of a percent. "All Irish-Born Five Points Residents" category is based on a random sample taken from the 1855 state census. Other categories based on information in the Emigrant Savings Bank Collection, Test Books and Ledgers, New York Public Library. The "All Irish-Born Five Points Bank Depositors" category is composed of all 192 Five Points Irish men and women who opened accounts among the bank's first 5,000 depositors. These accounts were opened from September 1850 to July 1853. The "Lansdowne" category includes information from all Lansdowne immigrants who opened accounts from 1851 through May 1855. For the vocations that comprise each occupational category, see n. 49.

the masons, and haul away the work-related debris. Municipal projects also employed many laborers, especially to dig sewer lines and pave streets. When outdoor work slowed in mid-summer, a laborer might find a job along the waterfront, loading or unloading sacks and crates from the hundreds of ships that plied New York City's waterways each week. Laborers' work was often very dangerous, and newspapers overflowed with reports of hod carriers killed in falls from ladders, longshoremen crushed by cargo, and laborers buried by the collapsing walls of unfinished buildings. A fair share of these work-related fatalities must have befallen Lansdowne families.⁵⁰

Dangerous as a laborer's work might be, his greatest fear was probably not death but unemployment. On days too cold or wet to work, the laborer did not get paid. Some might find steady employment at a single construction site; others had to look for a new position each day. Sudden sickness or a job-related injury could also throw one out of work at any time, since no position was held open for a laborer while he recuperated. Even in perfect health, observed the *New York Tribune* at this time, only "an energetic and lucky man . . . can make more than two hundred and fifty days' work as an out-door laborer in the course of a year, while the larger number will not average two hundred." During recessions, many laborers could not secure more than one or two days of paid work in a week. Such unemployment wreaked havoc on family finances. "A month's idleness, or a fortnight's sickness, and what misery!" observed an Irish journalist. Some Lansdowne immigrants left New York City permanently to search for steadier work. Upstate New York, Massachusetts, Ohio, and (to a lesser extent) Virginia were their favorite destina-

⁵⁰ For neighborhood construction accidents, see *New York Tribune*, December 4, 1850; *New York Herald*, November 5, 1853. The best work on laborers, although it does not consider them in the urban context, is Peter Way, *Common Labour: Workers and the Digging of the North American Canals, 1780–1860* (New York, 1993).

tions.⁵¹ Many others undoubtedly made seasonal journeys to seek employment, especially in the winter, when there was little work for the city's laborers, a practice the Lansdowne immigrants would have found familiar. The laborer's life was thus one of the hardest, most dangerous, and most financially precarious in Five Points, and for at least his first few years in New York, nearly every adult male Lansdowne immigrant found himself forced to take such work.⁵²

Children contributed significantly to the family coffers in the Lansdowne enclave, either to supplement their parents' unreliable incomes or to replace it in the case of households headed by widows. Many youngsters worked as newsboys. Tim Sullivan, the son of Lansdowne emigrants Daniel Sullivan and Catherine Connelly, lived at 25 Baxter (formerly Orange) Street in 1870 when at age seven he began hawking newspapers. The other trade most popular with Five Points boys was shoe shining. Sullivan, in fact, had shined shoes outside a police stationhouse before peddling newspapers. Bootblacks typically ranged in age from ten to sixteen, although some (such as Sullivan) started work much younger. Other boys earned money in more unusual ways. Many Five Points saloons featured bowling alleys, where Charles Dickens observed that young lads "wait upon the players, setting up the pins, returning the balls, fetching a light for their segars, supplying them with liquor when thirsty," and receiving in return a small wage from the saloonkeeper and tips from the bowlers. One such "pin boy" was Timothy Harrington, a Lansdowne immigrant who had left Kerry in the summer of 1851 with a brother and three sisters. When the fourteen-year-old opened a savings account in 1853 with an initial deposit of \$30 (about \$640 today), he was living with his seventeen-year-old brother John in the heart of the Lansdowne enclave at 155 Anthony Street. Girls had fewer opportunities to earn cash than did their brothers. Some made significant sums sweeping the mud away from street corners on rainy days, receiving tips in return from passersby. In the late summer and early fall, they might sell hot corn from steaming buckets. But girls were more likely to contribute to the family economy in other ways, especially scavenging coal or kindling from waterfront docks. Such work sustained many an impoverished Five Points family.⁵³

Lansdowne women also contributed to their families' incomes, and in the cases of widows and unmarried women sometimes supported their dependents single-handedly (see Table 3). The employment figures for Lansdowne women, however, are somewhat misleading. Most Lansdowne immigrants earned extra income taking

⁵¹ The dispersion of the Lansdowne immigrants can be followed to some extent by the advertisements they placed in the missing persons column of the *Boston Pilot*. See, for example, the ads of February 28, April 3, June 19, August 28, October 2, 16, 1852, April 9, 30, August 6, September 3, 10, October 1, 29, 1853. I am grateful to Ruth-Ann Harris of Boston College, who has indexed these advertisements by Irish parish of origin, for bringing them to my attention.

⁵² *New York Tribune*, May 8, 13, 14 (quotation), 1850; John F. Maguire, *The Irish in America* (London, 1868), 232–33.

⁵³ Edward W. Martin [James D. McCabe], *The Secrets of the Great City* (Philadelphia, 1868), 261–64; Owen Kildare, *My Mamie Rose* (New York, 1903), 45–51; "C. L. B." in *New York Times*, March 12, 1853; John Morrow, *A Voice from the Newsboys* (New York, 1860), 128–32; *Frank Leslie's Illustrated Newspaper* (December 29, 1855): 43; *New York Times*, October 16, 1902; Family 79, Sixth Election District, Sixth Ward, 1870 United States manuscript census (identified as Sullivan in Daniel Czitrom, "Underworlds and Underdogs: Big Tim Sullivan and Metropolitan Politics in New York, 1889–1913," *Journal of American History* 78 [1991]: 539–40); Charles Dickens, *American Notes* (1842; rpt. edn., London, 1985), 79; Account 5735, Emigrant Savings Bank Test Books. Harrington managed to add only \$5 more to his account before closing it in March 1854, just five months after he had opened it.

TABLE 3
Women's Employment by Occupational Category, 1851–1856

| | <i>Female Five Points Lansdowne Immigrants' with Bank Accounts</i> | <i>All Five Points Irishwomen</i> |
|-----------------------|--|---------------------------------------|
| Needle Trades | 7% | 48% |
| Household Servants | 52% | 25% |
| Laundresses | 28% | 8% |
| Boardinghouse Keepers | not available | 13% |
| Miscellaneous | 14% | 6% |

NOTES: All percentages rounded to nearest whole number. The "Lansdowne" category includes all Lansdowne immigrants who opened accounts at the Emigrant Savings Bank until August 1856. The "All Five Points Irishwomen" category is based on a random sample taken from the 1855 state census. The bank secretary failed to record the occupations of many women who did work for pay; see n. 54.

in boarders, an enterprise usually supervised by the female head of household, yet of the twenty-nine Lansdowne women who listed occupations when they opened bank accounts, none described themselves as boardinghouse keepers. Perhaps taking in boarders was so common in the Lansdowne enclave that neither the Lansdowne women nor the bank secretary considered it an "occupation."⁵⁴ Nor do the bank records reflect the full range of work Lansdowne women engaged in outside the home, for we know from other sources that some of them (usually widows or their children) turned to one of the most degrading of nineteenth-century occupations—ragpicking. Ragpickers scavenged the city's ash bins and rubbish barrels looking for rags, bones, scrap meat, and metal or glass that could be sold to paper producers, fat renderers, candle and soap makers, and other manufacturers.

One can only speculate on the extent to which Lansdowne women participated in the female trade most associated with Five Points—prostitution. Writing in 1849, just before the Lansdowne immigrants arrived, New York journalist George Foster said of Five Points that "nearly every house and cellar is a groggery below and a brothel above." A minister who worked with the neighborhood's most impoverished residents remembered that on his arrival there in 1850 "every house was a brothel, and every brothel a hell."⁵⁵

Could such accounts of the ubiquity of Five Points prostitution have been true, or were they merely the exaggerated fantasies of imaginative or bigoted writers? At first glance, the claim that "every house was a brothel" seems ridiculous. Yet a

⁵⁴ Women were listed as account holders on 105 of the 153 Lansdowne accounts. Of these, 48 were joint accounts opened by married couples, 11 were opened by married women who did not list their husbands as joint account holders, 5 were joint accounts with children rather than a husband, and 21 were opened by women who were widows or had never been married. The remaining 20 were second or third accounts opened by the same depositors. In all but one case, the bank secretary recorded employment information for women only when there was no husband or working-age son serving as a joint account holder. Of the 47 accounts opened by women that did not list male breadwinners as joint account holders, the bank secretary recorded occupations for 28 of them, wrote "none" for the occupation for 12 of them, and left the remainder blank.

⁵⁵ [George G. Foster], *New York in Slices: by an Experienced Carver, Being the Original Slices Published in the N.Y. Tribune* (New York, 1849), 23; letter of L. M. Pease in *New York Times*, November 19, 1852.

review of the New York district attorney's indictment records reveals that for the two blocks that composed the Lansdowne enclave, such allegations were essentially accurate. Twenty of the twenty-five dwellings on Anthony Street between Centre and Orange housed prostitutes at some point during the 1850s. Brothel proprietors were likewise prosecuted in thirteen of the seventeen residences on Orange Street just north of the Five Points intersection in the same period. Five Points policemen were actually renowned for ignoring the neighborhood vice trade. Prosecutions usually ensued only when neighbors repeatedly complained about persistently raucous behavior. Thus it seems likely that brothels were found in most Lansdowne tenement buildings during the early 1850s.⁵⁶

There is no evidence, however, that Lansdowne immigrants themselves were particularly active in the Five Points sex trade. Their common surnames—Sullivan, Shea, Sheehan, and Harrington—are noticeably absent from the Five Points indictment records. Census records also indicate that Lansdowne immigrants did not rent their tenements' commercial spaces, where most brothels operated under the guise of saloons and dance halls. Most indicted Five Points brothel keepers had lived in New York far longer than the typical Lansdowne immigrant. It is possible that Lansdowne women worked as prostitutes in brothels run by others, because it was the brothel *keepers* rather than their courtesans who were typically prosecuted. Yet while some Lansdowne immigrants may have worked as prostitutes, the evidence suggests that most lived among the neighborhood's brothels rather than working in them. Most Irish-American women who resorted to prostitution lived by themselves—most had emigrated on their own to work as domestics and had no family members to turn to for help when their financial circumstances became dire. Because almost all the Lansdowne immigrants had arrived in the United States with other family members, they had friends or kinsmen who could assist them in desperate times and were therefore less likely than other Five Pointers to become prostitutes. The Lansdowne immigrants, especially those with children, must have nevertheless found it embarrassing and humiliating to have to live in tenements where the sex trade was so brazenly and noisily carried on.

⁵⁶ In other sections of the Five Points neighborhood, prostitution was far less pervasive. In the same two decades, there were only two prosecutions for commercial sex in the eighty or so buildings on Mulberry Street below Bayard, only two indictments in the sixty buildings on Bayard Street from the Bowery to Orange, and none on Mott Street below Bayard. Prostitution in the Lansdowne enclave had decreased significantly by the mid-1850s due to the efforts of Protestant ministers who set up missions at the Five Points intersection. Brothel locations are based on a list compiled by Professor Timothy Gilfoyle, Loyola University of Chicago, in the possession of the author. Gilfoyle's inventory is based primarily on criminal indictments. I have examined most but not all of these records relating to Five Points. For prosecutions involving the Lansdowne enclave, see, for example, indictments of May 23, 1851 (151 Anthony and 6 Little Water Streets), January 23, 1852 (145 Anthony), April 22, 1852 (163 Anthony and 33, 35, 36 ½, 40, and 41 Orange), and April 23, 1852 (143, 149, and 157 Anthony), New York County District Attorney's Indictment Papers, New York Municipal Archives. These records must be used with some caution. While some of those arrested may have been innocent, notations on the indictment papers indicate that the overwhelming majority were convicted, pled guilty, or promised to abandon the premises (thus implying guilt). In addition, it was possible to be charged with conducting a "disorderly house" that was merely a raucous saloon rather than a brothel. After examining the indictments and the accompanying affidavits, however, I accept Gilfoyle's contention that the vast majority charged with this offense probably promoted prostitution.

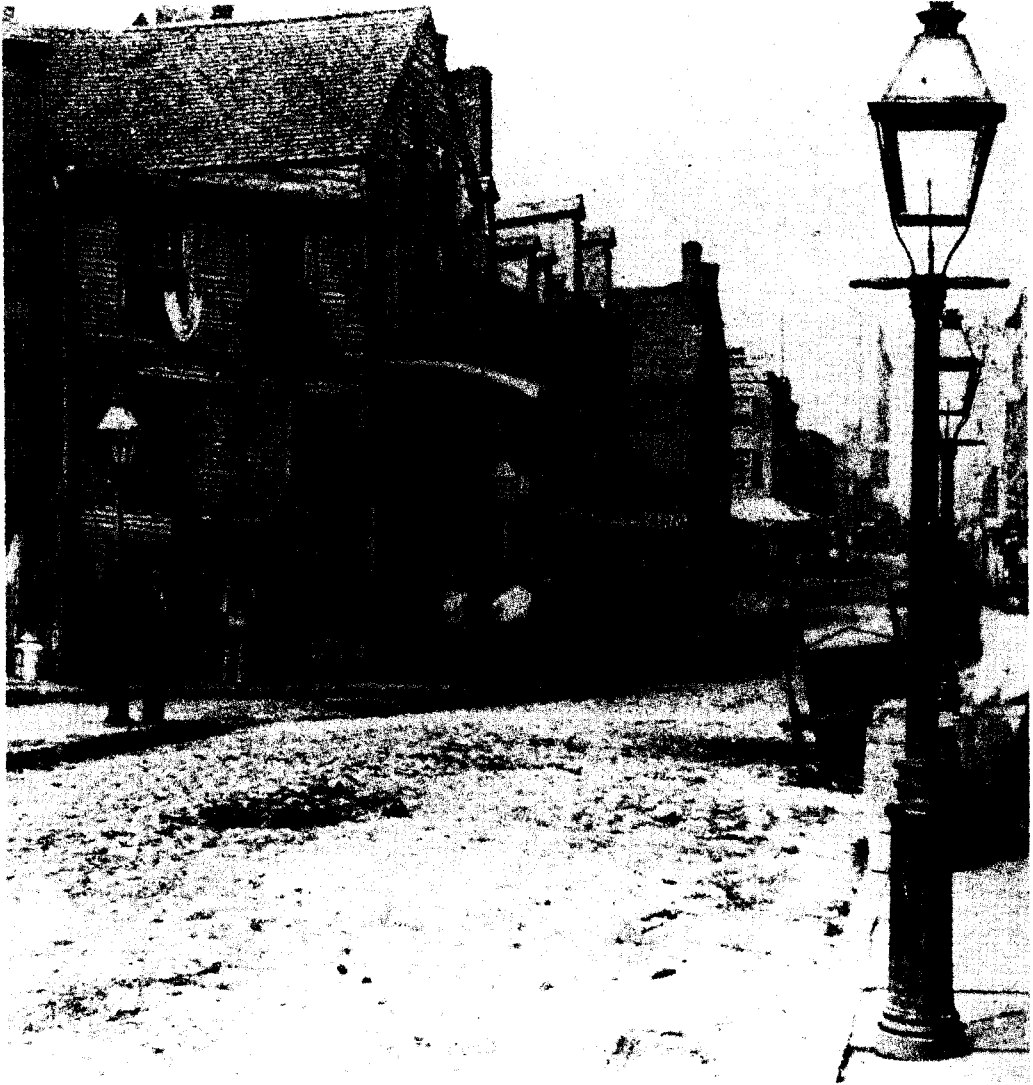


FIGURE 6: Infamous for its brothels, Anthony Street (renamed Worth in 1854), is shown here in the mid-1860s from the Five Points intersection facing west. This block, between Orange and Centre streets, was one of the two on which the majority of Lansdowne immigrants settled. In the building at the corner on the left, Bridget Mangin operated a well-known bordello. Andrew Crown's grocery and saloon occupied the ground floor. Courtesy of the New-York Historical Society.

FROM ALL APPEARANCES, THEN, the Lansdowne immigrants must have been some of the most impoverished residents of Five Points, a neighborhood renowned for its destitution. After all, the Lansdowne men were especially likely to be lowly paid menial laborers, and they lived with their families in the district's most rundown tenements. The Lansdowne immigrants' propensity to open accounts at the Emigrant Savings Bank makes it possible to compare these appearances with reality. Of the 12,500 accounts opened at the bank in its first six years of operation (from September 1850 to August 1856), 153 were opened by Five Points Lansdowne immigrants. Although it is difficult to determine what proportion of Lansdowne immigrants had accounts, it appears that about 50 percent of the Lansdowne

families living in Five Points had opened one by mid-1855. These account holders are not necessarily a representative sample, of course. It may be that only those who had found financial success bothered to open them. Even so, the bank records provide a rare glimpse into the economic fortunes of a very significant number of the Lansdowne immigrants.⁵⁷

The bank ledgers suggest that, even while living in Five Points, the Lansdowne immigrants saved far more money than one would have imagined given their wretched surroundings and low-paying jobs. Take the case of the three Tuosist natives who, on July 2, 1853, walked the half mile from Five Points to the bank's Chambers Street office to open accounts. The first, Honora Shea, had been one of the earliest Lansdowne-assisted immigrants to arrive in New York, disembarking from the *American Eagle* in mid-March 1851 with her daughter Ellen Harrington, who the bank secretary described as "an illegitimate child, aged 14 yrs." Although Honora apparently did not live with a male breadwinner, and rented quarters in the decrepit tenement at 35 Orange Street, she was able to open her account with an initial deposit of \$160, the equivalent of more than \$3,400 today. The next account was assigned to laborer Patrick Murphy and his wife Mary, who lived next door to Shea at 33 ½ Orange and had also arrived in New York from Tuosist in March 1851. They made an initial deposit of \$250, a sum worth roughly \$5,300 in contemporary terms. Bank officials gave the subsequent account to "washer" Barbara Sullivan, whose cramped apartment, filled with her six children, son-in-law, and six boarders, was described earlier. Sullivan, who at this point also lived at 33 ½ Orange but had arrived in New York in September 1851, made the smallest opening deposit of the three, \$135 (or about \$2,900 today). Later in the day, a fourth Tuosist native, forty-two-year-old Judy O'Neill, opened a joint account with her nineteen-year-old daughter Catherine (Judy was either widowed or never married). The O'Neills lived at 33 ½ Orange as well and had arrived in New York at the end of the initial Lansdowne flotilla in May 1851. Judy told the bank secretary that she was not employed outside the home but that her daughter worked in a tobacco factory. They made an initial deposit of \$148 (about \$3,100 today). These four Lansdowne families, who had probably arrived in New York virtually penniless, had managed despite their meager incomes to squirrel away *very* substantial sums.⁵⁸

These depositors opened their accounts with more money than the average Lansdowne immigrant, but not a lot more. The median initial bank deposit of the Lansdowne Five Points immigrants was \$100, while the average was \$102. This was slightly more than the \$96 initial deposit made by the average Five Pointer. Many Lansdowne immigrants, of course, started their accounts with just a few dollars and closed them a few weeks later, either because they needed the money or because

⁵⁷ Fully 152 of the 153 Lansdowne accounts were opened by immigrants who had arrived in New York by the time the state census was taken in June 1855. If we conservatively estimate the size of the average Lansdowne nuclear family to be four persons, then these 152 accounts may represent nearly 60 percent of the 1,000 Lansdowne immigrants in Five Points as of that date. (Some opened more than one account, however, which is why I put the figure at 50 rather than 60 percent.) That this estimate is fairly accurate is confirmed by a survey of the Lansdowne immigrants listed in the 1855 census as living on Orange Street above the Five Points intersection. Sixty percent of the Lansdowne households on this block had accounts at the Emigrant Savings Bank.

⁵⁸ Accounts 4737, 4738, 4739, and 4745, Emigrant Savings Bank Test Books and Account Ledgers; Glazier, *Famine Immigrants*, 7: 84 (where the O'Neills are listed as "Jude" and "Cath." Corkery).

they did not believe that their savings were safe in a bank. Only about half the depositors made significant additions to their accounts before closing them—just 51 percent of the Lansdowne immigrants ever managed to increase their initial balance by 50 percent or more. Many of the accounts were closed during the financial panics of 1854 and 1857. The Lansdowne immigrants' rush to withdraw all their money at once indicates that their runs on the bank were inspired by fear of the institution's failure rather than poverty brought on by these severe recessions.

It appears that most Lansdowne immigrants saw the bank as a place to safely keep (and draw interest on) nest eggs they had already managed to accumulate before opening their accounts. This would explain why so many Lansdowne immigrants did not add substantially to their initial deposits, even when they kept their accounts open for extended periods. Bonane native Mary Flynn, for example, was in her early sixties when she opened an account in August 1853. Although she was married, her husband was not listed as a joint account holder. Mary, who told the bank secretary that she had no occupation, opened her account with \$45, and in less than a year she had doubled her money. During the recession winters of 1855 and 1858, she withdrew as much as half her savings but always worked her way back to the \$90 level within a year. That was the balance, give or take five dollars, at which her account remained into the late 1860s. Flynn undoubtedly saw \$90 as the appropriate size for her family's emergency fund.⁵⁹

In 28 percent of the Lansdowne accounts, the immigrants accrued \$250 or more—\$5,300 in modern terms—substantial financial resources for famine-era immigrants. (Only 22 percent of non-Lansdowne Five Pointers ever reached a balance of \$250 or more in their accounts.)⁶⁰ Laborer Dennis Sullivan, for instance, a widower with three children and a native of Cloonee townland in Tuosist, opened an account in February 1854 with a \$98 deposit. Despite the two intervening recessions, Sullivan had increased his savings to more than \$300 (\$6,400 today) by 1858. Laborer Murty Sullivan, born just a mile or two down the road from Dennis in the Tuosist townland of Ardea, was also an avid saver. Murty arrived in New York in April 1852, opened his bank account in February 1853, and when the panic of 1857 induced him to close it, he had amassed \$367.90 (\$7,800 today). Bonane native Daniel O'Connor and his wife Honora opened their account in May 1853. When the couple's balance peaked in 1860, it had reached nearly \$450 (about \$9,600 today). Laborer Timothy Shea and his wife Johanna resided with their four children on Anthony Street when he opened his account in December 1852 with \$112. By the time the Sheas closed it in July 1860, the Kenmare natives had accumulated \$495 (more than \$10,500 today). A second Timothy and Johanna Shea from Kenmare, who arrived in New York in 1853, had amassed \$592 (nearly \$12,600 in contemporary terms) by July 1857. A third Timothy Shea (sometimes called "O'Shea"), this one a "bill carrier" who had emigrated from Tuosist at age forty-eight in 1851, managed along with his wife Honora to save \$658 (about \$14,000 today) in three accounts by July 1857, the highest sum attained by any of the Lansdowne immigrants among the bank's first 12,500 depositors. The three Tim

⁵⁹ Account 5155, Emigrant Savings Bank Test Books and Account Ledgers.

⁶⁰ The highest balance achieved by Lansdowne immigrants was, on average, exactly \$200. The highest balance reached by non-Lansdowne Five Pointers was, on average, \$234.

Sheas and their spouses had fared amazingly well for men and women who had been on the brink of starvation at the beginning of the decade.⁶¹

What explains the Lansdowne immigrants' surprising ability to save? Perhaps the privation they had experienced in County Kerry enabled them to limit expenditures to the bare minimum in the years immediately after their arrival. Living in Five Points, they could pay among the lowest rents in New York (from \$4.50 to \$6 per month for a two-room dwelling), and taking in their countrymen as lodgers enabled the Lansdowne immigrants to recoup a significant proportion of their housing expenses. Having so many of their kinsmen and former neighbors with them in New York also undoubtedly helped the immigrants. Virtually overnight, they created a large, intricate network that could be used to help find jobs, housing, and even spouses. And if someone through sickness, injury, or death became unable to work, there were plenty of relatives around to help out. There were many Lansdowne immigrants who did not fare as well as the three Tim Sheas. For widows with young children, life was particularly hard. But the noticeable absence of Lansdowne surnames in the relief records of the Five Points Mission (where hundreds of desperate Five Points Catholics turned for help despite the institution's Methodist affiliation) suggests that the Lansdowne immigrants took care of each other—helping widows find new mates and unemployed men and women new jobs.

But what explains the apparent anomaly of the Lansdowne immigrants' menial jobs and poor living conditions on the one hand and their substantial bank balances on the other? It appears that, once they got settled and found work, the Lansdowne immigrants focused all their energies on saving money to establish nest eggs for their families, choosing to stay in Five Points even after they could afford to move to more spacious apartments in cleaner and safer neighborhoods. Inasmuch as many of them were also sending money to loved ones in Ireland, either to help support aged parents or to pay for relatives' emigration, the typical Lansdowne immigrant's ability to squirrel away \$100 or more in just a few years is truly remarkable. Some undoubtedly moved out of Five Points or to less squalid blocks within the neighborhood once they had established these competencies. But many, despite their substantial savings, decided to stay in the Lansdowne enclave, either because they enjoyed being surrounded by so many fellow Kerry natives, or because they sought to continue saving as much money as possible by paying low Five Points rents. That so many of the Lansdowne immigrants' account balances remained relatively constant indicates that once they reached their savings goal, they began to raise their standard of living by spending more of their income. Through a concerted scheme of self-sacrifice, then, most of the Lansdowne immigrants in Five Points managed to improve their lives significantly, from both the misery of County Kerry and the initial privations of Five Points.

⁶¹ For a list of all the accounts opened by Lansdowne immigrants during the bank's first six years of operation, see Anbinder, *Five Points: The Nineteenth-Century New York Neighborhood That Invented Tap Dance, Stole Elections, and Became the World's Most Notorious Slum* (New York, 2001), 465. The accounts described in this paragraph are 1235, 3424, 3735, 4409, 6473, 7464, 12046, and 12419. Occupations are based on information given at the time an account was opened. By the time the high balance was achieved, the account holders may have moved on to more lucrative employment.

EVENTUALLY, THE LANSDOWNE IMMIGRANTS did begin to move out of Five Points, but it is impossible to determine exactly how long they remained in the district or where they went once they left. There were simply too many Sheas and Sullivans (and even Ellen Hollands) in New York to allow the tracking of the Lansdowne immigrants after they moved out of the neighborhood. That young bootblack and newsboy Tim Sullivan was living in Five Points in 1870 demonstrates that some of them still resided there, but by then Italian immigrants occupied the vast majority of the apartments in the buildings that Lansdowne immigrants had once dominated. At that point, most Lansdowne immigrants had probably moved further uptown to other, more prosperous Irish enclaves.

We do know that the Lansdowne immigrants had undergone a remarkable transformation in a few short years from destitute Irish peasants to moderately successful New Yorkers. In Ireland, they had been among the most wretched of that nation's inhabitants. The Lansdowne tenants survived six years of unrelenting hardship in Ireland during the famine, a period of privation far longer than that of the typical famine-era emigrant. In a period when hundreds of thousands of desperate and impoverished Irish men, women, and children fled to North America, the Lansdowne immigrants were singled out by observers on both sides of the Atlantic for their particularly dire circumstances. Many apparently perished in the "Lansdowne Ward" of New York Hospital, so-called because many of the marquis's former tenants died there soon after their arrival.⁶² Yet while these immigrants may have first settled in Five Points because they could afford nothing else, they chose to stay even after they could have easily moved to more respectable neighborhoods. In just a few years in New York, many of these once "unfortunate creatures" achieved a modicum of financial security. While Trench may have exaggerated the immigrants' fortunes when he talked of them returning with chains of gold, by 1860 a large number of the immigrants had in fact saved enough to purchase such baubles had they chosen to do so.

The fate of young Tim Sullivan, the bootblack and newsboy, reflects the myriad opportunities that New York offered to the Lansdowne immigrants and their children. In his various jobs delivering newspapers, Sullivan developed a network of contacts among the city's newsboys and periodical dealers. Even though by his teenage years he worked in the news plants themselves, he simultaneously became a newspaper distributor, because the distribution managers knew that Sullivan, through his web of newsboys, could guarantee that their papers would be sold throughout the city. "Every new newspaper that come out, I obtained employment on, on account of my connection with the news-dealers all over the City of New York," Sullivan recalled in 1902. His income from these operations must have been significant, because by his late teens he was ready to open his first saloon, and by his early twenties he purportedly had interests in three or four. Sullivan was also very popular, and at twenty-three, without any prior legislative experience, he was elected to the New York state assembly. Although first chosen for office as an

⁶² For the Lansdowne Ward, see *Dublin Review*, n.s., 12 (January–April 1869): 4–17; *Tralee Chronicle*, February 26, 1869; and *Freeman's Journal*, November 20, 1880, all quoted in Lyne, *Lansdowne Estate*, 92–94; Charles Russell, "New Views on Ireland," or, *Irish Land: Grievances: Remedies*, 3d edn. (London, 1880), 47; Lansdowne, *Glanerought and the Petty-Fitzmaurices*, 129.

insurgent running against the city's "Tammany Hall" Democratic organization, "Five Points Sullivan" soon began cooperating with Tammany and quickly moved up through the ranks. He eventually served in the state senate and the United States House of Representatives. By the turn of the century, this child of Lansdowne immigrants had become "Big Tim" Sullivan, "the political ruler of down-town New York." Some observers considered him the second most powerful politician in the city, after Tammany "boss" Richard Croker. Sullivan also became quite wealthy. Critics charged that his fortune had been built from payoffs exacted from gambling and prostitution syndicates in his district. But "Big Tim" insisted that he had never taken a bribe in his life and that his substantial income derived from shrewd investments in vaudeville theaters and other legitimate businesses. No matter what the origin of his fortune may have been, Sullivan remembered his humble origins and shared his wealth with his less prosperous constituents, giving away thousands of pairs of shoes and Christmas dinners each year.⁶³

Sullivan's allies also remembered their leader's roots. When the city decided to cut a new street through a tenement district about a mile north of Five Points to ease the flow of traffic to the recently opened Williamsburg Bridge, the Board of Aldermen voted to name the new thoroughfare Kenmare Street. The lawmaker who proposed the name told the press in 1911 that he had chosen it in honor of Sullivan's mother, a Kenmare native, because she had "exercised a motherly care over me as she did over hundreds of boys on the east side."⁶⁴ It is impossible to determine if the alderman in question truly owed this debt of gratitude to Mrs. Sullivan or merely sought to curry favor with his principal political patron. Whatever the case, Kenmare Street was christened in the very week that, exactly sixty years earlier, Ellen Holland had first set foot in New York.

In that span of time, the Kenmare immigrants had risen from civic embarrassments to city leaders. Whether the Lansdowne immigrants' bank accounts were typical, and to what extent their particularly harrowing pre-migration experiences affected their ability to save, is a matter of conjecture. Only when historians have analyzed both the financial and biographical data in the thousands of non-Lansdowne Emigrant Savings Bank accounts will we be able to grasp the full significance of Ellen Holland's story. Yet whatever such future studies may reveal, a few things are certain. First, the degree of financial success achieved by the Lansdowne immigrants despite their decrepit surroundings suggests that the famine immigrants adapted to their surroundings far better and more quickly than we have previously imagined. After the initial year or so of adjustment, the Lansdowne immigrants stayed in Five Points not because they had to but because they chose to. In addition, the Lansdowne immigrants' story demonstrates the value of tracing the lives of famine-era immigrants back to Ireland, adding a transatlantic perspective that has generally been lacking in the field of immigration history. That Five Pointers from the Lansdowne estate achieved their modicum of financial security despite the extraordinary hardships (even by Irish standards) that they faced before,

⁶³ *New York Sun* (April 18, 1889): 5; *New York Herald* (October 16, 1902): 5; (May 19, 1907), magazine sect., part 1, pp. 1–2; *New York Times* (April 30, 1887); (October 16, 1902): 3; Czitrom, "Underworlds and Underdogs," 541–42.

⁶⁴ *New York Times* (March 2, 1911): 2.

during, and immediately after their arrival in New York makes those monetary accomplishments all the more remarkable. Their saga demonstrates that we still have a lot to learn about how nineteenth-century immigrants adjusted to—and were transformed by—life in modern America.

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Of Civilization and Savages: The Mimetic Imperialism of Japan's 1874 Expedition to Taiwan

ROBERT ESKILDSEN

IN THE SPRING OF 1874, a Japanese military expedition departed the Japanese port of Nagasaki bound for southern Taiwan. According to government documents, the expedition had two purposes: an ostensible purpose of punishing a group of Taiwanese aborigines responsible for the murder of fifty-four people from Ryūkyū (present-day Okinawa), who were shipwrecked in southern Taiwan late in 1871, and a real purpose of establishing colonies on eastern Taiwan in order to civilize the savage inhabitants of the region. The government denied in public that the expedition had any colonial intent, and under foreign pressure decided to postpone the expedition, but the commander in charge of the Japanese forces, holding previously issued imperial orders, preempted the government by dispatching his troops to Taiwan before they could be recalled. The expedition subsequently achieved its ostensible purpose, and Japanese forces punished a group of Taiwanese aborigines in a brief series of violent battles. Almost five months after the fighting ceased, a languorous series of negotiations in Beijing produced an inconclusive diplomatic settlement, and a month later the expeditionary force withdrew from Taiwan and returned home, without having achieved its publicly unacknowledged purpose of colonization.

If we looked no further than government documents, the story of the Taiwan Expedition would seem to end there. Commercially published sources—newspapers, woodblock prints, and *kawaraban*, crude monochrome prints similar to European broadsides—show there is more to the story. In particular, they show the intimate connection between Japanese imperialism and Japan's vexed encounter with Western civilization, and they shed light on how the selective appropriation of Western civilization and the projection of Japanese military force abroad contributed to the formation of modern identity in Japan. In recent years, European historians have offered increasingly sophisticated appraisals of the contributions that colonial empires made to modern life in European metropolises, but Japanese historians have barely begun to consider the close relationship between Japan's colonial empire and modernity in Japan. Indeed, in a recent review article, one scholar has criticized historians in the field of Japanese history for failing to pay attention to how Japan's colonies—especially Korea—contributed to modern

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Japanese identity.¹ He argues, among other things, that Japan did not export modernization to its colonies after it had accomplished its own modernization but, rather, that Japanese colonialism happened concurrently with and contributed much to Japan's modernizing process. The discourse on civilization and savagery that gained popularity at the time of the Taiwan Expedition points to a similar pattern. Even before Japan established a formal colonial empire, debates about using Japanese military power overseas drew heavily on the imagery and rhetoric of Japan's own efforts at modernizing. Despite being shot through with contradictions and ambivalence, the idea of exporting the Western civilizing impulse to the indigenous population of Taiwan helped justify, naturalize, and explain the concurrent effort to modernize Japan. Mimesis of Western imperialism, in other words, went hand in hand with mimesis of Western civilization.

In order to elucidate the connection between the mimesis of imperialism and civilization in Japan, this essay will describe how the attempt to export the Western civilizing impulse to Taiwan constituted part of Japan's concurrent appropriation and adaptation of Western civilization. To that end, the explanation that follows will proceed in five parts. First, it will describe how the Japanese strategy of appropriating and adapting selected aspects of Western civilization, which took place during a period of upheaval and rapid change, helped define the historical context in which the Taiwan Expedition took place. Second, it will explain the specific colonial logic of bringing civilization to the "savages" of Taiwan, a legalistic definition of political jurisdiction that the Japanese government used as a way of justifying the colonization of Taiwan. Third, it will examine how commercially published sources about the expedition exaggerated the savagery of the Taiwanese aborigines, thereby exaggerating the cultural difference that separated them from the Japanese. Fourth, it will examine how commercial sources portrayed Japanese dominance over the aborigines as an instrument that could improve Japan's status in a world order dominated by the West. Finally, it will examine how commercially published representations of the submission of the aborigines diverged from the government's rationale for the expedition and stressed instead the importance of establishing Japanese authority as a first step in exporting civilization to the "savages."

THE STRATEGY OF SIMULTANEOUSLY appropriating and exporting Western civilization came easily in Japan, because Westerners self-consciously pursued imperialism in the name of civilization, and because, for Japan, the prospect of exporting civilization to Taiwan provided an attractive means of resisting Western imperialism. In effect, commercial representations of the expedition simply extended an existing strategy of resisting imperialism by appropriating Western civilization so that it included the idea of exporting civilization to Taiwan. To interpret commercial representations of the expedition this way shares an affinity with the approaches used in some postcolonial studies, but only to the extent that a postcolonial point

¹ Andre Schmid, "Colonialism and the 'Korea Problem' in the Historiography of Modern Japan: A Review Article," *Journal of Asian Studies* 59, no. 4 (2000): 951–76.

of view helps explain the adaptation of Western discourse for the purpose of resisting it.² Indeed, many readers may see a postcolonial interpretation of the Taiwan Expedition as ironic at best, since Japan never suffered Western colonization and since the expedition intended to colonize Taiwan, not to mention the fact that Japan later went on to establish a colonial empire. Japan's role in imperialism is not as clear-cut as these facts suggest, however, and historians disagree about how to characterize Japan's status—as colonizer or as “semi-colonized”—in the second half of the nineteenth century.³ The historiographical disagreement about Japan's status points to a key ambiguity about Japan's role in imperialism, but it is not necessary to debate here whether Japan was a victim or a perpetrator of imperialism. Rather, it is important instead to recognize that Japanese efforts to appropriate and adapt Western ideas about power and hierarchy pervaded Japanese society at the time, that these efforts offered a means of resisting Western assertions of Japanese inferiority, and that Japan's mimesis of Western imperialism took place in this context.

Because it involved mimesis of Western imperialism, the Taiwan Expedition surely bears some relationship to the origins of Japanese imperialism, although this, too, remains a matter of debate. Again, the debate need not be resolved here, but the question of origins nevertheless deserves a few words if only to highlight more clearly how the expedition fit into the historical context of Japan during the 1870s. In their explanations of how Japanese imperialism began, historians have overwhelmingly devoted their attention to the Sino-Japanese War (1894–1895) and to the establishment of formal external colonies in Taiwan and Korea, in 1895 and 1910 respectively.⁴ Few historians have sought to explain the Taiwan Expedition as an example of Japanese imperialism,⁵ and the historiography has tended to treat the

² For one example, see Partha Chatterjee, *Nationalist Thought and the Colonial World: A Derivative Discourse*, 2d edn. (Minneapolis, 1993). Historians have made postcolonial arguments similar to the one being made here about Japan's colonial relationships in the early twentieth century. For one example, see Kim Brandt, “Objects of Desire: Japanese Collectors and Colonial Korea,” *positions: east asia cultures critique* 8, no. 3 (2000): 711–46.

³ Many scholars in Japan, following a more-or-less Leninist interpretation of the spread of industrial capitalism, have characterized Japan's relationship with the West in the second half of the nineteenth century as semi-colonial, in particular by placing it in the context of the spread of British imperialism from India to China and Japan. On the other hand, historians also stress Japan's role as a colonial power in Taiwan and Korea (with formal annexation in 1895 and 1910 respectively). For a review of the historiography about Japan's opening to the West, see Tanaka Akira, “Meiji ishin no sekaiteki kankyō o dō toraeru ka,” in Sasaki Ryūji, ed., *Sōten Nihon no rekishi* (6): *Kin-gendai hen* (Tokyo, 1991), 14–25. Recent scholarship in Japan has tended to follow Katō Yūzō's interpretation that Japan's position in the East Asian treaty port system was less unequal than China's because China signed unequal treaties as the result of losing a war, whereas Japan signed them as the result of negotiated agreements. Katō, *Kurofune zengo no sekai* (Tokyo, 1985).

⁴ W. G. Beasley, *Japanese Imperialism, 1894–1945* (Oxford, 1987); Mark R. Peattie, “Introduction,” in Ramon H. Myers and Mark R. Peattie, eds., *The Japanese Colonial Empire, 1895–1945* (Princeton, N.J., 1984), 3–52. Peter Duus, on the other hand, describes Japanese policy toward Korea before the Sino-Japanese War as an example of informal empire, and he argues that Japan changed to a strategy of formal empire in Korea because of the failure of Korean reform and continued Chinese weakness in the context of the “new imperialism.” Duus, *The Abacus and the Sword: The Japanese Penetration of Korea, 1895–1910* (Berkeley, Calif., 1995), 29–65.

⁵ Mōri Toshihiko, *Taiwan shuppei* (Tokyo, 1996); Leonard Gordon, “Japan's Abortive Colonial Venture in Taiwan, 1874,” *Journal of Modern History* 37, no. 2 (1965): 171–85; Hilary Conroy, *The Japanese Seizure of Korea, 1868–1910: A Study of Realism and Idealism in International Relations* (Philadelphia, 1960). Marlene Mayo rejects Gordon's characterization of the expedition as an “abortive

incident as a bilateral diplomatic conflict between China and Japan that clarified Japanese claims to sovereignty over the Ryūkyū Kingdom and Chinese claims to sovereignty over Taiwan.⁶ Some historians have argued that the internal colonization of Hokkaido marks the beginning of Japanese colonialism,⁷ and the fact that Hokkaido was colonized around the time of the Taiwan Expedition raises questions about the connection between the expedition and internal colonization.

These various historiographical perspectives on the origins of Japanese imperialism point to a number of key issues of interpretation, but they provide little help in explaining the colonialism of the Taiwan Expedition. The establishment of Japan's formal colonial empire has served as an influential historiographical guidepost, but it also encourages the view that Japanese colonialism happened after Japan had accomplished its own modernization, rather than that colonialism and modernization happened concurrently, and this has created a historiographical blind spot about the colonial dimension of the Taiwan Expedition. Similarly, the view of the expedition primarily as a bilateral diplomatic dispute between China and Japan, or as a conflict that clarified their borders, relies on a top-down national frame of analysis that excludes from consideration the domestic reaction to the expedition, which provides a number of important clues about how people in Japan also understood the expedition as a military operation and as an expression of Japanese dominance. Nor does the internal colonization of Hokkaido or Okinawa raise the same issues about Japan's willingness to project military power abroad in order to increase its standing in the world. These various historiographical perspectives cannot explain, in other words, how people in Japan used the Taiwan Expedition as an opportunity to assert, rather aggressively, Japan's new status in the world, nor how their aggressive assertions grew out of Japan's engagement with Western civilization in the middle of the nineteenth century.

It may seem unremarkable, viewed from our perspective today, that in the 1870s people in Japan would worry about Japan's standing in the world, but in a number of ways their concern represented an important departure from past practice, and it helped define the unusual character of Japanese imperialism. The Meiji Restoration, a palace coup followed by a brief civil war, brought an end to the Tokugawa *bakufu*, or shogunate, which had governed Japan during the Edo period (1603–1867), and it inaugurated a revolutionary transformation of Japan's political,

colonial venture" and sees it instead primarily as a response to samurai unrest. M. J. Mayo, "The Korean Crisis of 1873 and Early Meiji Foreign Policy," *Journal of Asian Studies* 31, no. 4 (1972): 818.

⁶ Kō Sekai, "Taiwan jiken (1871–1874)," *Kikan kokusai seiji* 28, no. 2 (1964): 38–52; Kinjō Seitoku, *Ryūkyū shobun ron* (Naha, Okinawa, 1978); Kuribara Jun, "Taiwan jiken (1871–1874 nen): Ryūkyū seisaku no tenki toshite no Taiwan shuppei," *Shigaku zasshi* 87, no. 9 (1978): 10–85; Ishii Takashi, *Meiji shoki no Nihon to higashi Ajia* (Yokohama, 1983); Shu Cherin, "Gaisei to tōchi: 1874 nen Taiwan shuppei o rei ni shite," in Gendai Kenpōgaku Kenkyūkai, ed., *Gendai kokka to kenpō no genri* (Tokyo, 1983), 389–416; Ichinose Norie, "Meiji shoki ni okeru Taiwan shuppei seisaku to kokusaihō no tekiyō," *Hokudai shigaku* 35 (1995): 23–49. For accounts of the Taiwan Expedition from Chinese perspectives, see T. F. Tsiang, "Sino-Japanese Diplomatic Relations, 1870–1894," *Chinese Social and Political Science Review* 17 (1933): 1–106; Sophia Su-fei Yen, *Taiwan in China's Foreign Relations, 1836–1874* (Hamden, Conn., 1965); D. R. Howland, *Borders of Chinese Civilization: Geography and History at Empire's End* (Durham, N.C., 1996), 35–41.

⁷ For an example, see Richard Siddle, *Race, Resistance and the Ainu of Japan* (London, 1996). Siddle criticizes Peattie's view of Japanese colonialism (cited above) because it ignores Japan's earlier development of Hokkaido, which Siddle sees as an example of internal colonization.

social, economic, and intellectual order that dominated the early decades of the Meiji period (1868–1912). The Meiji Restoration thus brought to an end the era of government dominated by the samurai class, and it nominally restored the emperor to a central role in governance, although samurai authority and imperial authority existed in an uneasy tension until the ideology of imperial authority became more firmly established in the 1880s.⁸ The revolutionary transformation of the Meiji period also involved a radical reformulation of what civilization meant in Japan, and this impinged on a wide range of practices and beliefs, including how people looked at Japan's place in the world. Until the 1850s and 1860s, when the old order began decisively to pull apart due to a variety of indigenous and exogenous causes, any number of explanations could be invoked to assert Japan's political, cultural, or spiritual superiority, and none of them paid much attention to what the rest of the world thought.⁹ Such explanations did not disappear overnight following the Meiji Restoration, but Western explanations gained predominance quite rapidly in the 1860s and 1870s. The close link in Western thinking between civilization and a global order of nations, and the way these two ideas were received in tandem in Japan, accounts for the abrupt shift. In effect, the strategic appropriation of Western civilization offered a way of contesting Japan's low status in a Western-dominated global order of nations. Appropriating Western civilization for this purpose quickly became one of the defining features of the period.

To be sure, the instrumental use of Western civilization to resist Western assertions of Japanese inferiority began before the Taiwan Expedition. By the mid to late 1860s, a number of Japanese intellectuals had already begun to extol the virtues of Western civilization, but the mid-1870s marked a high point in the *bunmei kaika* ("Civilization and Enlightenment"), a broad effort, bordering on a fetish at times, to introduce selected aspects of Western civilization into Japan. Not everyone in Japan accepted Eurocentric influence on Japanese culture, though, and opposition to it contributed to rural revolts and samurai rebellions during the 1870s.¹⁰ Indeed, domestic opposition to the *bunmei kaika* underscores the fact that Japan's reception of Western civilization took place in the context of resistance to the West. The *bunmei kaika* amounted to an important domestic reaction both to Western civilization and to Western imperialism, and commercial representations

⁸ Carol Gluck, *Japan's Modern Myths: Ideology in the Late Meiji Period* (Princeton, N.J., 1985); Takashi Fujitani, *Splendid Monarchy: Power and Pageantry in Modern Japan* (Berkeley, Calif., 1996).

⁹ Some explanations involved adaptations of Sinocentric views of the world, recentered from China onto Japan, while others involved views of Japanese divinity that derived from the sacred nature of the imperial house, the martial tradition of the samurai class, or the inherent divinity of the land itself. Ronald P. Toby, *State and Diplomacy in Early Modern Japan: Asia in the Development of the Tokugawa Bakufu* (1984; Stanford, Calif., 1991), 55–64, 89, 170; Bob Tadashi Wakabayashi, *Anti-Foreignism and Western Learning in Early-Modern Japan: The "New Theses" of 1825* (Cambridge, Mass., 1986), 17–57; Harry D. Harootunian, "The Functions of China in Tokugawa Thought," in Akira Iriye, ed., *The Chinese and the Japanese: Essays in Political and Cultural Interactions* (Princeton, N.J., 1980), 10–17; Asao Naohiro, "Higashi Ajia ni okeru bakuhan taisei," in Asao, ed., *Sekaishi no naka no kinsei* (Tokyo, 1991), 113–14, 122. Questions of national prestige began to take on greater importance in Japanese political discourse around the time of the Perry Expedition of 1853–1854. See, for example, a memorial by Ii Naosuke of 1853 in which he argues that Japan's "courage and prestige resound beyond the seas." W. G. Beasley, ed. and trans., *Select Documents on Japanese Foreign Policy, 1853–1868* (London, 1955), 117–19.

¹⁰ Stephen Vlastos, "Opposition Movements in Early Meiji, 1868–1885," in John W. Hall, et al., eds., *The Cambridge History of Japan*, 6 vols. (Cambridge, 1988–99), 5: 367–431.

of the Taiwan Expedition took images and ideas of the *bunmei kaika* and extended them to apply to international matters. The expedition thus took place in the context, and was interpreted through the lens, of Japan's engagement with Western civilization, wherein a strategic appropriation of civilization could be used as a way of contesting Japan's status in the Western-dominated international order.

The circumstances of how Japan engaged Western civilization as a response to Western imperialism had important consequences. For one thing, the reproduction of Western civilization existed in tension with an antipathy to its corrosive effects on Japanese culture, and for better or for worse Western imperialism left Japan little choice but to engage Western civilization, which meant that the corrosion could not be avoided. For another, the strategy of appropriating Western civilization in order to challenge Western imperialism and improve Japan's status in the world involved weakening the middle ground between civilization and savagery. That middle ground—semi-civilized status—might under other circumstances have served as a basis for solidarity between the Japanese and other East Asian peoples. Japan's strategy thus foreclosed the possibility of solidarity with the rest of East Asia in order to boost Japan's status. Fukuzawa Yukichi, one of the foremost intellectuals of the age and an early champion of the *bunmei kaika*, evoked these themes in an 1885 essay entitled "On Escaping Asia" (*Datsu-A ron*). In it, he likened Western civilization to a measles epidemic—a fatal scourge that plagued Japan repeatedly during the nineteenth century—because it spread in the same inexorable manner as an epidemic, although infection by Western civilization brought benefits as well as harm. Further, Fukuzawa warned against Japanese solidarity with China and Korea because, as he saw it, if Japan became associated with such weak and backward countries, it would only drag Japan into permanent subservience to the West. He predicted that China and Korea would both succumb to the onslaught of Western civilization because they would not embrace being infected by it as Japan had. In other words, partly as a means of survival and partly to gain the benefits of Western civilization, Japan must accept the corrosive influence of the West, and it must escape solidarity with the rest of East Asia.¹¹ In 1874, commercial sources about the Taiwan Expedition prefigured Fukuzawa's famous formulation. They showed that Japan's involvement in Taiwan emulated Western methods of domination, but with key modifications that served a dual purpose. First, the modifications elevated Japan's standing in the Western-dominated international order, and, second, they eliminated the middle ground between civilization and savagery that might trap Japan in an inferior status to the West. As in Fukuzawa's famous essay, commercial sources about the Taiwan Expedition evince a sense of Japan's vulnerability to Western imperialism and an awareness that Western civilization could be adapted to mitigate its vulnerability. They show that mimesis of civilization went hand in hand with mimesis of imperialism, and both worked for distinctly Japanese purposes.

¹¹ Fukuzawa Yukichi, "Datsu-A ron," *Jiji shinpō*, March 16, 1885, in Ishida Takeshi, ed., *Kindai Nihon shisō taikō 2: Fukuzawa Yukichi shū* (Tokyo, 1975), 510–12.

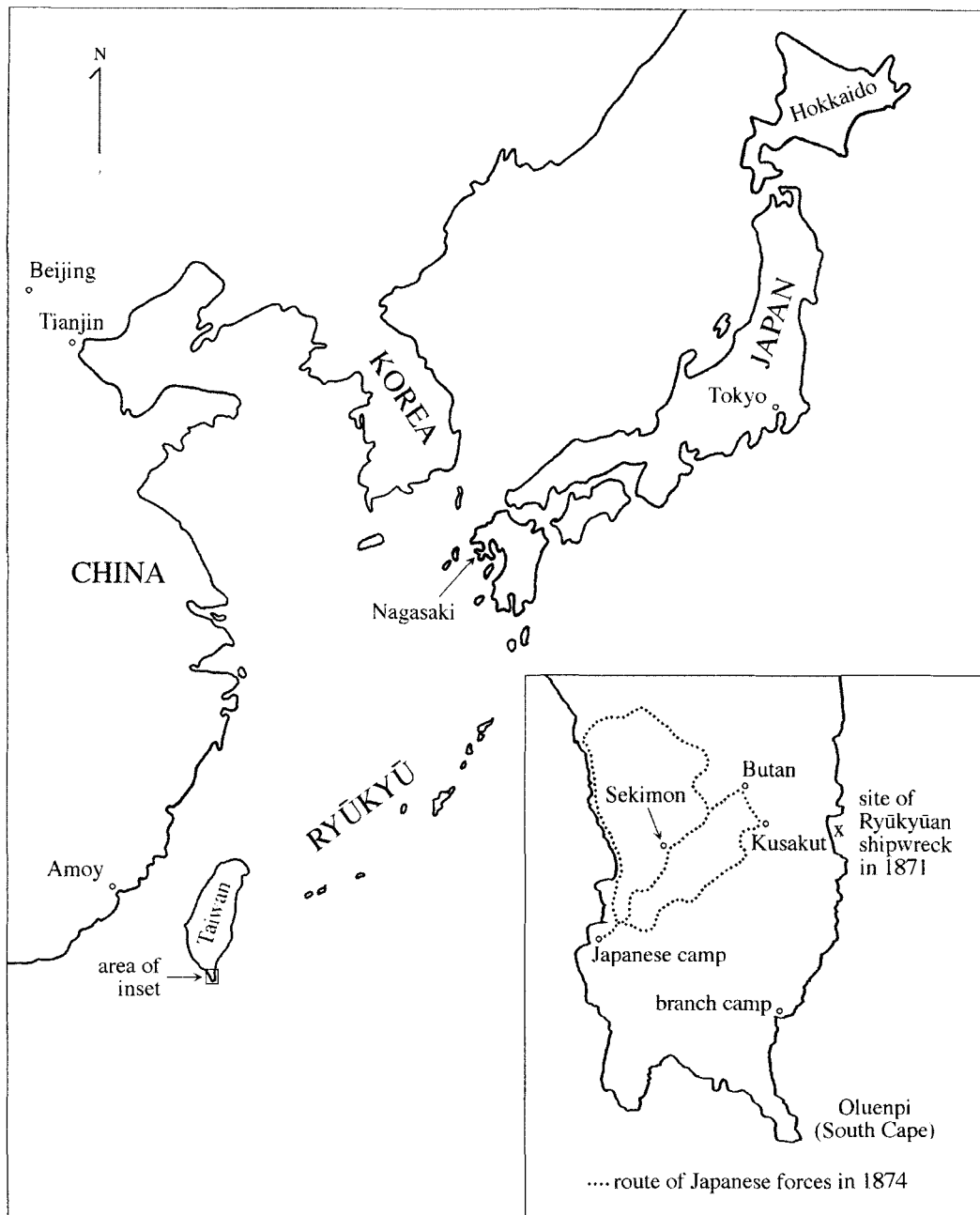


FIGURE 1: Map of East Asia around the time of the Taiwan Expedition.

A LEGALISTIC EXPLANATION OF CIVILIZATION AND SAVAGERY informed the colonial logic that the Japanese government employed during the Taiwan Expedition, and it derived from a theory of sovereignty and jurisdiction devised by Charles LeGendre during the years he spent as American consul in Amoy. LeGendre left the U.S. diplomatic service in 1872, joined the Japanese government as an adviser, and over the next several years wrote a number of plans for the Japanese government that

called for Japan to colonize eastern Taiwan in the name of bringing civilization to the savages.¹²

Before working for the Japanese government, LeGendre served as American consul in Amoy from 1866 to 1872, and as part of his duties he sought redress from Chinese officials for the murder of several American castaways by a group of Taiwanese aborigines in 1867. During his negotiations with the Chinese, LeGendre argued that, under the terms of the Treaty of Tianjin (1858), an “unequal treaty” that gave Western powers unreciprocated legal and economic rights in China, the Chinese government had an obligation to exercise its jurisdiction over the aboriginal territory and to punish the acts of the aborigines. Moreover, he argued from international law that, unless China sustained the exercise of its jurisdiction over the aboriginal territory, in other words unless China civilized the savages who lived there, it could not claim sovereignty over the territory. In an attempt to motivate the Chinese to action, LeGendre repeatedly raised the threat that unless China exercised effective jurisdiction over the territory, a foreign power would colonize it.¹³

The recalcitrant stance of the Chinese authorities frustrated LeGendre. They countered his argument by claiming that China held sovereignty over the entire island of Taiwan but that the terms of the Treaty of Tianjin did not apply to the aboriginal territory because it lay outside the boundaries of Chinese legal jurisdiction, and they steadfastly refused to take action to exercise jurisdiction over it.¹⁴ The conflict between LeGendre and the Chinese proved impossible to resolve for several reasons. An unequal treaty, a quintessential feature of Western imperialism in East Asia, provided the legal basis for LeGendre’s repeated claims against the Chinese government, and Chinese officials rarely wasted an opportunity to dispute the applicability of the unequal treaties. The two sides also used different meanings of sovereignty, one deriving from Chinese practice and the other from European, and this disagreement created a virtually irreconcilable conflict about what jurisdiction over the aboriginal territory meant. Finally, Chinese officials knew from experience that bringing the aboriginal territory to heel, as LeGendre demanded, would come at an unacceptably high price in money and human lives, and they concluded that inaction posed less of a risk than action.¹⁵ In 1872, LeGendre left his

¹² Charles LeGendre, Memo No. 22, March 13, 1874, Ōkuma monjo C426, Waseda University Library (hereafter, “Ōkuma monjo”). For more on LeGendre’s role, see Emily Howard Atkins, “Charles W. LeGendre and the Japanese Expedition to Formosa, 1874” (PhD dissertation, University of Florida, 1953); Sandra Carol Taylor Caruthers, “Charles LeGendre, American Diplomacy, and Expansionism in Meiji Japan” (PhD dissertation, University of Colorado, 1966); Yen, *Taiwan in China’s Foreign Relations*.

¹³ Charles LeGendre, “Notes of Travel in Formosa,” 4 vols., unpublished mss, 1874, 3: 293–98, 354–84, Papers of Charles William LeGendre, Library of Congress (hereafter, LeGendre Papers). For a more detailed explanation of how LeGendre’s experiences on Taiwan contributed to the colonialism of the Taiwan Expedition, see Robert Eskildsen, “Meiji nananen Taiwan shuppei no shokuminchiteki sokumen,” in Meiji Ishinshi Gakkai, ed., *Meiji ishin to Ajia* (Tokyo, 2001), 61–95.

¹⁴ Yen, *Taiwan in China’s Foreign Relations*, 131, 140, 145–46.

¹⁵ Yen, *Taiwan in China’s Foreign Relations*, 127–28, 133, 146, 152–53; John Robert Shepherd, *Statecraft and Political Economy on the Taiwan Frontier, 1600–1800* (Stanford, Calif., 1993). Shepherd argues a similar general point that Qing dynasty authorities adopted a strategy of minimizing military conflict and monetary cost in their effort to contain violence along the border between the Chinese settler population and the aboriginal territory in Taiwan during the eighteenth century.

position as consul at Amoy without having resolved the dispute, and he set sail from Amoy, intending to return to the United States.

On his way back, LeGendre stopped off in Yokohama, where the American minister in Japan introduced him to Japan's foreign minister, Soejima Taneomi. LeGendre impressed Soejima with his knowledge of southern Taiwan, and Soejima hired him to advise the Japanese government about how to deal with problems raised by the massacre of the Ryūkyūans in 1871. At Soejima's request, LeGendre prepared a number of memoranda about the Taiwan problem. The memos outlined a plan for Japanese annexation of the eastern part of the island, and they gave Soejima a compelling rationale for establishing Japanese colonies there.¹⁶ In his memos to Soejima, LeGendre used the same logic he had used with the Chinese—that China had no valid claim to sovereignty over eastern Taiwan because it did not exercise legal jurisdiction over the aboriginal territory. As unclaimed land, eastern Taiwan could be claimed by any nation willing to undertake the burden of civilizing it, including Japan. In this way, LeGendre introduced into Japanese government discourse the idea, derived from international law, that bringing civilization to the “savages” of Taiwan justified colonizing the territory. Not all Europeans and Americans agreed with LeGendre's interpretation of Taiwan's status, but they attacked his argument on the grounds that it might cause war, which would harm Western commercial interests in East Asia, or that he had dismissed too lightly Chinese claims to sovereignty over eastern Taiwan. They did not attack his general argument that territory inhabited by savages could be claimed in the name of civilizing it.¹⁷ Indeed, LeGendre seems to have done little more than transmit to the Japanese a decades-old Western interest in colonizing Taiwan.¹⁸

Soejima and several other leaders left the government late in 1873 after a dispute about whether Japan should invade Korea, but the remaining leaders continued to pursue the goal of colonizing eastern Taiwan based on LeGendre's rationale. A few members of the government, such as Kido Takayoshi, opposed the colonial plan because they wanted to use Japan's resources to address domestic problems rather than to waste them on a foreign entanglement of uncertain outcome,¹⁹ but no records exist that suggest that anybody in the government opposed colonization per se. By the spring of 1874, advocates of the expedition had smoothed over the opposition enough to begin making concrete preparations. On March 13, 1874, LeGendre submitted his final expedition plan to Ōkuma Shigenobu, the Japanese official who had taken charge of planning for the expedition after Soejima left the government. LeGendre's memorandum stated that the Japanese government intended to “civilize the whole aboriginal population” and that, in order not to provoke unnecessary foreign opposition, the “ostensible object” of the expedition would be to punish the aborigines who had murdered the

¹⁶ Memos No. 1–3, Waseda Daigaku Shakai Kagaku Kenkyūjo, ed., *Ōkuma monjo*, 6 vols. (Tokyo, 1958), 1: 17–33; Memo No. 4, “Ōkuma monjo” A4424.

¹⁷ Mōri, *Taiwan shuppei*, 134–35; Ichinose, “Meiji shoki ni okeru Taiwan shuppei,” 32–35.

¹⁸ George Williams Carrington, *Foreigners in Formosa, 1871–1874* (San Francisco, 1977), 71–78, 89–102; Yen, *Taiwan in China's Foreign Relations*, 100–09.

¹⁹ Sidney Devere Brown and Akiko Hirota, trans., *The Diary of Kido Takayoshi*, 3 vols. (Tokyo, 1983), 3: 15, 93.

Ryūkyūans in 1871, while “its real object will be the annexation of Aboriginal Formosa.”²⁰

In the middle of March, Japanese officials began to take steps to implement the colonial plan. Ōkuma and several other government officials drew up a list of objectives for the expedition,²¹ and they began to lay the groundwork for the administrative apparatus that would control it. On April 5, Prime Minister Sanjō Sanetomi, acting in the name of the Japanese emperor, issued orders to Saigō Tsugumichi, the military leader of the expedition.²² Saigō’s orders spelled out the two purposes of the expedition, though with a different emphasis than LeGendre had given them in the plan he had presented to Ōkuma. The first clause of Saigō’s orders, corresponding to LeGendre’s “ostensible object,” stated that the primary purpose of the expedition would be to pacify the aborigines, while the second clause, similar to LeGendre’s “real object,” stated, “The purpose is to lead the natives gradually to civilization [*yūdō kaika seshime*], eventually establishing a profitable enterprise between them and the Japanese government.” The second clause added an important limitation, however: it ordered Saigō to send the government a detailed report about China’s response to the expedition and to ask for further orders before he could implement the step of civilizing the natives. In other words, Saigō had to receive further orders before he could actually begin to establish colonies along the east coast. The Japanese government may have been willing to colonize eastern Taiwan, but it would not allow irreversible action without reassessing the risk of war with China beforehand. In early April, the government established a Bureau of Savage Affairs (Banchi Jimukyoku), called the “Colonization Office” in official English translations, and it appointed Ōkuma as the “Minister of Colonization.”²³ LeGendre, acting on Ōkuma’s orders, hired personnel in Amoy who would help in establishing colonies along the east coast of Taiwan.²⁴ Someone, it is not clear who or when, wrote a detailed list of objectives for the expedition that went far beyond the plan outlined in LeGendre’s memorandum. It called for stationing military colonists at several points along the east coast, and it laid out a plan for troops to establish small branch camps that would form the basis of a permanent presence in the aboriginal territory.²⁵ The plan to colonize Taiwan, in full swing at least through the middle of April 1874, bore LeGendre’s imprint, but Japanese actors had also contributed a great deal to its formulation and implementation.

Leaks about the expedition plagued the government in April, and the expedi-

²⁰ “Ōkuma monjo” C426.

²¹ Saigō Totoku Kabayama Sōtoku Kinen Jigyō Shuppan Iinkai, ed., *Saigō totoku to Kabayama sōtoku* (Taihoku, 1936), 58–59.

²² Gaimushō Chōsabu, ed., *Dai Nihon gaikō bunsho*, vol. 7 (Tokyo, 1939), 19–20.

²³ Ishii, *Meiji shoki no Nihon to higashi Ajia*, 47; Ōkuma to LeGendre, April 7, 1874, LeGendre Papers.

²⁴ Telegram from LeGendre to Manson, March 24, 1874, LeGendre Papers.

²⁵ *Saigō totoku to Kabayama sōtoku*, 63–69. Various government documents refer to “military colonists” and “military colonies,” in some cases glossing the Japanese characters in phonetic script (for example, *miriteri koronī*) to indicate the English-language provenance of the term. The planned military colonists differed from the *tondenhei*, unemployed samurai who resettled to Hokkaido and provided combined military and police protection for the area, although plans for the *tondenhei* were formulated and implemented around the time of the Taiwan Expedition. Rinji Teishitsu Henshūkyoku, ed., *Meiji Tennō ki*, 13 vols. (Tokyo, 1968–77), 3: 274.

tion's colonial purpose seems to have been an open secret among the foreign community throughout the treaty ports of East Asia, including Yokohama. Reports in Japanese newspapers also made veiled references to the plan to annex eastern Taiwan, but with only a few exceptions Japanese newspapers exercised restraint—probably due to self-censorship—in describing the colonial intent of the expedition.²⁶ Terashima Munenori, Japan's new foreign minister, publicly denied that the expedition had any colonial intent, but the British and U.S. ministers to Japan remained skeptical and, fearing that the expedition would cause war with China, intervened as forcefully as they could in order to prevent it from proceeding.²⁷ Under foreign pressure, the Japanese government decided to postpone the expedition, but at the end of April, Saigō Tsugumichi preempted the government by dispatching his troops before they could be recalled. A variety of sources show that, for several months after the expedition had arrived on the island, its leaders continued to see their purpose as the colonization of eastern Taiwan.²⁸

The government apparently never made a formal decision to set aside the colonial plan, but after a series of meetings in early July, it shelved its plans to colonize eastern Taiwan because it did not want to risk war with China, which would have invited an unwelcome and potentially dangerous intervention by Western powers.²⁹ In sum, the government formulated a plan to colonize eastern Taiwan, it took concrete action to implement the plan, and it backed away from the plan not because it thought better of colonization but rather because it thought it could not afford a war with China and the risk of Western intervention that war would invite. Colonization remained a possibility, though a distant one, throughout the lengthy negotiations that finally resolved the dispute between China and Japan in the final months of 1874. Japanese negotiators never abandoned their fundamental position that for China to claim sovereignty over eastern Taiwan it must exercise effective jurisdiction there. From beginning to end, therefore, the Japanese government's rationale for intervening in Taiwan rested on the colonialist logic that for any government—Chinese, Japanese, or Western—to claim sovereignty over eastern Taiwan, that government must bring civilization to the savages.³⁰

²⁶ *Japan Daily Herald* (hereafter, *JDH*), April 11, 17, May 8 (quoting from the Nagasaki-based *Rising Sun*), May 11 (quoting from the Shanghai-based *North China Herald*); *Kōbun tsūshi* (hereafter, *KT*), April 20; *Yūbin hōchi shinbun* (hereafter, *YHS*), April 16. A notable exception to the muted coverage of the expedition's colonial plans in Japanese newspapers is an article by Kishida Ginkō in the May 15 edition of the *Tokyo nichinichi shinbun* (hereafter, *TNS*). Unless otherwise noted, the year for citations from newspapers is 1874.

²⁷ Ishii, *Meiji shoki no Nihon to higashi Ajia*, 48–60.

²⁸ Cassel to LeGendre, May 26, 1874, "Ōkuma monjo" C453; Saigō to LeGendre, June 7, 1874, LeGendre Papers; Kabayama Sukenori diary entry, May 8, 1874, *Saigō totoku to Kabayama sōtoku*, 320.

²⁹ Memorandum by Yamagata Aritomo, July 1874, "Ōkuma monjo" A169; Ōkuma to Sanjō, June 28, 1874, and Government Order on July 9, 1874, *Nihon Shiseki Kyōkai*, ed., *Ōkuma Shigenobu kankei monjo*, 6 vols. (Tokyo, 1932–35), 2: 396–99; *Meiji Tennō ki*, 3: 279–80. Reservations about going to war with China constrained Japanese plans for the expedition from the start and contributed to the clash in 1873 that precipitated Soejima's departure from the government. The government's change of heart in July 1874 thus represented a small shift by a divided government in a finely calibrated attempt—ongoing for several years—to press forward with the expedition without provoking war with China.

³⁰ "Memorandum of the Course of Action Taken towards the Aboriginal Territory of Formosa" (No. 7), LeGendre Papers.

COMMERCIAL SOURCES ACCENTUATED THE SAVAGERY of the Taiwanese aborigines in a striking manner that contrasts with the generally bloodless description of them in government documents. Unlike the narrow, legalistic contrast between civilization and savagery that the government used to justify the colonization of Taiwan, commercial sources, especially newspaper reports, proffered florid descriptions of the savagery of the aborigines in a way that underscored Japan's status as a civilized nation. Exaggerating the savagery of the aborigines also had the effect of evacuating the middle ground between civilization and savagery—semi-civilized status—that many Westerners believed Japan occupied at the time, so the exaggeration did more than simply foreclose the possibility of solidarity with the aborigines, it also implicitly challenged the Western view of Japan as semi-civilized. Stressing Japan's status as a civilized nation and denying the possibility of solidarity with the aborigines played on the hierarchical possibilities inherent in Western imperialism, but the hierarchical implications of aboriginal savagery would not necessarily have been clear to most Japanese at the time. As a result, commercial sources explained the implications by using analogies to Chinese concepts or to Japanese concepts from the Edo period, either of which would have been more accessible to most readers than Western concepts.

In the months and weeks before the expeditionary force departed, newspapers regularly exaggerated the aborigines' violent nature, characterizing them as cruel, inhuman, and ignorant of civilization (sometimes expressed by the term *kyōka*) or as lacking ethics (the Confucian *jinri*). Many of the articles heaped further criticism on the aborigines by accusing them—unjustly—of cannibalism. To be sure, during the preceding 250 years, foreigners had often been denigrated or mocked in Japan for their lack of civilization, but not even strident xenophobes had made savagery the defining characteristic of foreign barbarism. The unprecedented emphasis on savagery should, therefore, command our attention.

Many of the early newspaper reports about the expedition reproduced Chinese categories in their descriptions of the aborigines, following the Chinese distinction between “mature savages” and “raw savages” (or, more properly, “barbarians”). For example, the *Yūbin hōchi shinbun* of April 16, 1874, described the aborigines as follows:

The eastern part [of Taiwan] is inhabited entirely by natives [*dojin*]. Those among them who have attained a measure of civilization [*kyōka*] are called mature savages [*jukuban*]. They regularly engage in trade with the Chinese and they can understand each others' languages. They are not violent by nature. The next group are called raw savages [*seiban*]. There are about 200,000 of them. This kind [of savage] trades with the mature savages, but knows little of ethics [*jinri*]. [Also] known as native savages [*doban*], they comprise the eighteen savage races [*banshu*]. They are wild and rapacious, have large bodies and are very strong.

According to this view, civilization, violence, trade, and language served as a matrix that organized the aborigines hierarchically. The “mature savages” commanded more respect than the “raw savages” because they had Chinese trading partners and were not violent. The “mature savages” had also attained a degree of civilization, from China rather than Europe it should be noted, and the report described their partial civilization as *kyōka*, a term used during the first half of the nineteenth

century either to describe the Japanese effort to civilize the Ainu, an indigenous people living at the northern periphery of Japan,³¹ or to describe the effort to elevate other presumably inferior peoples, such as peasants.³² The article thus gave credit for the partial civilization of the “mature savages” to Chinese civilization and explained it by analogy to an older Japanese term for cultural transformation. Violence remained the most pronounced characteristic of the “raw savages,” and it gave fuller meaning to their lack of ethics, understood in Confucian terms. Unlike the partial civilization of the “mature savages,” however, the violence of the “raw savages” had no obvious antecedents in Edo-period descriptions of subordinate or inferior peoples.³³ Commercial sources such as this newspaper report thus used more familiar concepts of hierarchy and cultural difference to give meaning to the newly important characteristic of violence that defined the aborigines’ savagery.

Commercial sources about the expedition also used exaggerated reports of cannibalism to stress the savage nature of the aborigines. The aborigines practiced headhunting, to be sure,³⁴ but not cannibalism, and the earliest reports of cannibalism appear to have been inspired by Western expectations that the aborigines engaged in that quintessentially uncivilized act. A number of early articles about the expedition in expatriate newspapers described the alleged cannibalistic practices of the aborigines, such as an article in the *China Mail* that predicted the failure of the expedition despite Japan’s military advantage because the “cannibals” knew the land better and could travel light by “liv[ing] off the carcasses of their prisoners!”³⁵ Many of the articles in expatriate newspapers voiced skepticism about reports of cannibalism, however, such as an article in the *Japan Daily Herald* that cast doubt by reporting that the aborigines “are figuratively called ‘the cannibals,’” but “[t]here is no authentic record of any Formosan . . . having actually dined off their unfortunate victims.”³⁶ Compared to expatriate newspapers, Japanese newspapers exaggerated reports of cannibalism among the aborigines and rarely voiced skepticism. For example, the *Kōbun tsūshi* picked up the account from

³¹ Siddle, *Race, Resistance and the Ainu of Japan*, 40.

³² In the Edo period, Confucian scholars distinguished samurai learning from peasant learning. Ogyū Sorai, for example, distinguished between the self-cultivation of the samurai (expressed as *manabu*, to learn, or *utsuru*, to change) and the transformation of the general population that was imposed by a higher authority (*kassuru*, to change). Samuel Hideo Yamashita, trans., *Master Sorai’s Responsals: An Annotated Translation of “Sorai sensei tōmonsho”* (Honolulu, 1994), 93 n. 107. In the 1870s, the term *kyōka* conveyed a similar hierarchical understanding of the cultural transformation of an inferior brought about by a superior. According to Sheldon Garon, the leaders of modern Japan inherited the idea of *kyōka*, which he translates as “moral suasion,” and used it systematically on a nationwide scale as a tool for paternalistic social management. Sheldon Garon, *Molding Japanese Minds: The State in Everyday Life* (Princeton, N.J., 1997).

³³ The two most important Edo-period sources that taxonomize foreigners are Nakamura Tekisai’s *Kinmō zui* (1666) and Terajima Ryōan’s *Wakan sansai zue* (1713). Neither of them stress savagery or violence as an important characteristic of barbarian peoples. After their subjugation by Japanese forces in the seventeenth century, the Ainu were not particularly prone to violence or uprisings. Richard Siddle, “Ainu History: An Overview,” in William W. Fitzhugh and Chisato O. Dubreuil, eds., *Ainu: Spirit of a Northern People* (Seattle, 1999), 68–71; David L. Howell, *Capitalism from Within: Economy, Society, and the State in a Japanese Fishery* (Berkeley, Calif., 1995), 27–35.

³⁴ George Taylor, “Formosa: Characteristic Traits of the Island and Its Aboriginal Inhabitants,” in Glen Dudbridge, ed., *Aborigines of South Taiwan in the 1880s* (Taipei, 1999), 154–55; Frank M. LeBar, *Ethnic Groups of Insular Southeast Asia*, 2 vols. (New Haven, Conn., 1972–75), 2: 133; Miyamoto Nobuto, *Taiwan no genjūminzoku: Kaisō—watashi no minzokugaku chōsa* (Tokyo, 1985), 169–88.

³⁵ *China Mail*, reported in *JDH*, May 21.

³⁶ *JDH*, April 7.



FIGURE 2: Sensai Eitaku, book illustration, *Meiji taiheiki*, vol. 8, 1876. Historiographical Institute, University of Tokyo.

the *Japan Daily Herald* mentioned above and reported that the aborigines lived “by eating the meat of the people they defeat in battle” (*shisha no niku o kutte*), in the process erasing the doubt raised in the original report.³⁷

Even after credible firsthand reports appeared in Japanese newspapers explaining that the aborigines did not practice cannibalism, commercially published accounts of the expedition—especially books—continued to accentuate the savagery of the aborigines by describing them as cannibals.³⁸ Figure 2 is an illustration from *Meiji taiheiki*, a popular history of the Meiji period published two years after the expedition, that shows how persistent the view of the aborigines as cannibals became. Reports of cannibalism persisted partly because they buttressed assertions of Japan’s status as a civilized nation but also because they conformed to older associations from the Edo period. Expressions of cannibalism had a long history in Japan, including one influential account that was originally inspired by sixteenth-century European reports of Brazilian cannibalism. Mention of foreign cannibals that derived from this account remained confined for hundreds of years, however, to rare and conventionalized references to Brazilians who ate the flesh of men and lived in burrows in the ground.³⁹ In 1874, expatriate newspaper reports often

³⁷ *KT*, May 25.

³⁸ *YHS*, June 27; Tashiro Mikio, *Taiwan gunki* (1874).

³⁹ The characteristic phrasing can be traced to the 1602 Beijing edition of a map of the world by the Jesuit scholar Matteo Ricci; Japanese *jinbutsuzu*, diagrams of peoples of the world, preserved the

mentioned cannibalism among the aborigines but never mentioned cave dwelling; by contrast, Japanese commercial sources often added references to cave dwelling to their reports about Taiwanese cannibals that had been translated from Western newspapers. Commercial sources thus blended and accentuated the alleged cave-dwelling and flesh-eating practices of the aborigines and touted their savagery so effectively that cannibalism endured as a defining feature of Taiwanese savagery for years after the expedition.

The way that commercial sources accentuated the savagery of the aborigines stands out as striking. It broke dramatically from past practice, and it contrasted almost as dramatically from the more subdued portrayal of the aborigines in government documents. While commercial sources clearly accentuated the savagery of the aborigines, they offer little evidence to explain why they did so. The systematic and persistent nature of the references to aboriginal savagery show, however, that the exaggeration could not have been an accident or an anomaly. To be sure, the aborigines of southern Taiwan had a bad reputation among the Chinese settlers in the area, and endemic violence punctuated the uneasy relationship between the two populations. Japanese sources picked up on Chinese reports of violence, just as they picked up on sensationalized Western reports of the aborigines' cannibalism, but they pushed these characterizations further than Chinese or Western sources did. The fact that Japanese commercial sources remain silent about why they exaggerated the aborigines' savagery makes it difficult to draw hard and fast conclusions, but one effect of the exaggeration is clear: it increased the perceived cultural distance that separated the Japanese from the aborigines. In the context of the 1870s, a larger cultural distance helped both to validate Japanese claims for higher status in the Western-dominated international order and to eliminate a middle ground between civilization and savagery that might trap the Japanese in a less than salutary solidarity with other East Asian peoples.

THE IDEA OF USING FORCE as an instrument to establish Japanese dominance in Taiwan became a popular theme in commercial representations of the Taiwan Expedition, more popular than the theme of aboriginal savagery, even though it was far from clear what Japanese dominance over the aborigines meant. When Japanese forces landed in southern Taiwan in 1874, they sought first and foremost to establish military dominance over the aborigines, and commercial sources gave prominent place to descriptions of the military action. They did not portray dominance as an end in itself, however, stressing instead that dominance would bring Japan prestige in the world. They suggested, in other words, that dominance could be used as an instrument to increase Japan's status and power. Viewing force as an instrument that Japan, as opposed to a Western civilized power, could wield in this way constituted an important modification of Western imperialism, but while

phrasing for two and a half centuries. L. Carrington Goodrich, "China's First Knowledge of the Americas," *Geographical Review* 28, no. 3 (1938): 406; Robert Eskildsen, "Telling Differences: The Imagery of Civilization and Nationality in Nineteenth Century Japan" (PhD dissertation, Stanford University, 1998), 82–98.

the use of force to establish dominance over the aborigines appears to have enjoyed broad support, commercial sources provide abundant evidence that no consensus existed about what constituted the proper political basis of Japanese dominance. Viewed from another angle, in the years after the Meiji Restoration, a consensus had not yet been reached about what constituted the proper basis of political authority in Japan, and the confusion about political authority found expression in the way that commercial sources portrayed Japanese dominance over the aborigines.

The confusion about political authority centered on overlapping debates about two related questions: should the samurai class or the emperor be the preeminent symbol of political authority, and should Japan embrace Western civilization or expel it? Debates about both issues contributed to the formation of modern national identity in Japan, and both issues lay at the center of the political transition that took place during the Meiji Restoration. Considering the contested nature of the Restoration and the gradual nature of identity formation, it should come as no surprise that both political authority and national identity remained confused in the 1870s. More surprising, however, is the close connection between mimetic imperialism and debates about domestic political power and national identity: the way that commercial sources depicted Japanese dominance over the aborigines suggests that mimetic imperialism was implicated in the complex process of redefining political power and national identity as Japan engaged Western civilization in the 1870s. Mimetic imperialism, in other words, did not result from Japan's engaging Western civilization; rather, it constituted part of the process of Japan's engaging it, and for that reason mimetic imperialism helped shape national identity and the new political order in the Meiji period. In order to elucidate how these overlapping debates informed representations of dominance in commercial sources about the Taiwan Expedition, the discussion below will first explain how Japanese forces established military dominance over the aborigines, then offer an example of a commercial source that portrayed dominance as an instrument that could improve Japan's status.

Soon after the Japanese expeditionary force arrived on Taiwan, it sought to establish military dominance over the aborigines through a series of aggressive strikes. Newspaper articles and accounts of the expedition by participants provide compelling evidence that the fighting proved to be one-sided and short, if not exactly easy. The fighting began when a group of aborigines ambushed a small Japanese scouting party on May 18. Using matchlock rifles, they shot to death two Japanese soldiers and, in their tradition of headhunting, took the head of one of the Japanese dead before they retreated into the mountains.⁴⁰ Within a few days, Japanese forces mounted a retaliatory strike, and on May 22 a major battle took place at a ravine that the Japanese sources called Sekimon (literally, Stone Gate). The Japanese suffered four killed and twelve wounded, while the aborigines suffered seventy killed and wounded. In the samurai tradition, Japanese soldiers took the heads of several of the dead, including the leader of the Butan and his son. A few days later, Saigō Tsugumichi ordered a major assault on the people of Butan

⁴⁰ Ochiai Yasuzō, *Meiji shichinen seiban tōbatsu kaikoroku* (Tokyo, 1920), 74–76; *TNS*, June 3; *YHS*, June 13; *JDH*, June 23.

and Kusakut, the two villages suspected of participating in the slaughter of the Ryūkyūan castaways in 1871, and the assault took place between June 1 and June 3. Following the recommendation of his American military advisers, Saigō split the Japanese force into three units: the first carried out a frontal assault against the Butan, the second set out with a Gatling gun in tow to attack the Kusakut (impassable roads compelled them to send the gun back to camp before they had traveled far), and the third performed a flanking maneuver, proceeding north along the coast before heading over the mountains to attack the Butan from the rear. By the time the fighting ended, the villages inhabited by the Butan had been burned to the ground, as had several other villages in the area, and the Butan and Kusakut had been scattered. By the middle of July, the chiefs of all the aborigine villages of southern Taiwan had presented themselves at the expeditionary headquarters and “submitted” to Japanese authority.⁴¹

A few months later, woodblock print artists produced a number of prints that depicted the fighting.⁴² By the 1870s, the woodblock print publishing industry had existed in Japan for nearly two centuries, but the advent of newspapers and new printing technologies after the Meiji Restoration had forced print publishers to adapt. Many print publishers and print artists went into the newspaper business,⁴³ the most famous example being the print artist Ochiai Yoshiiku, who helped found the *Tokyo nichinichi shinbun* and who probably drew most of the illustrations for that newspaper.⁴⁴ Print artists and publishers also tried to survive by exploiting the success of newspapers, taking popular themes from newspapers and transforming them into catchy prints—called newspaper prints (*shinbun nishikie*)—that probably sold more for their sensationalist impact than for their news value.⁴⁵ The *Tokyo nichinichi shinbun* carried the most influential reports about the expedition, and many of the prints about the expedition purported to take their information from that newspaper. One of the most famous prints about the Taiwan Expedition, a triptych by Yoshiiku from October 1874, depicts the fighting as a glorious victory for the Japanese (Figure 3), and it provides an example of the sensationalistic nature

⁴¹ Ochiai, *Meiji shichinen seiban tōbatsu kaikoroku*, 80–105; *TNS*, June 3, 6, 12, 25, 26; *YHS*, June 13; *JDH*, June 10, 23.

⁴² In addition to the triptych by Ochiai Yoshiiku shown in Figure 3, Tsukioka Yoshitoshi produced a print of the battle of Sekimon (reproduced in Segi Shin'ichi, *Tsukioka Yoshitoshi gashū* [Tokyo, 1978], plate 74), and Nagashima Mōsai produced a triptych about it. Nagashima's triptych and an additional six-print composition about the battle by Yoshiiku (possibly two triptychs) are listed in a catalog of items displayed at the Japanese colonial library on Taiwan in 1932, but the prints are no longer extant. Taiwan Sōtoku-fu Toshokan, ed., *Meiji nanannen seitaeiki kankei shiryō tenkan mokuroku* ([Taihoku], 1932), 14.

⁴³ Hara Hideshige, “Shinbun nishikie to nishikie shinbun,” *Kindai Nihon to jōhō*, Kindai Nihon Kenkyūkai, ed. (Tokyo, 1990), 68–92; Ono Hideo, *Nishikie shinbun* (Tokyo, 1972); Tsuchiya Reiko, *Osaka no nishikie shinbun* (Tokyo, 1995), 19.

⁴⁴ Ōnishi Hayashigorō, *Nihon shinbun hatten shi—Meiji Taishō hen* (Tokyo, 1995), 45. Because Yoshiiku helped found the *Tokyo nichinichi shinbun*, many scholars conclude that he probably did the illustrations for the newspaper, too; Tan'ō Yasunori, Tsuchiya Reiko (personal communication with the author).

⁴⁵ Japanese scholars remain divided over whether newspaper prints sold for their sensationalist impact or for their news value; the difference may be partly geographical, with Tokyo prints being more sensationalist and Osaka prints being more news oriented. Satō Kenji, “Shinbun nishikie to nishikie shinbun,” in Kinoshita Koreyuki and Yoshimi Shun'ya, eds., *Nyūsu no tanjō: Kawaraban to shinbun nishikie no jōhō shakai* (Tokyo, 1999), 14–16; Tsuchiya Reiko, “Nishikie shinbun to wa nanika,” in Kinoshita, et al., *Nyūsu no tanjō*, 102–04; Tsuchiya, *Osaka no nishikie shinbun*.



FIGURE 3: Ochiai Yoshiiku (October 1874), "Tokyo nichinichi shinbun nanakyaku jūni gō" (Tokyo Daily Newspaper, No. 712). Special Collections Room, Waseda University Library.

of newspaper prints. The journalist Okada Jisuke, an associate of Yoshiiku who also worked for the *Tokyo nichinichi shinbun*, wrote the text in the cartouche in the upper left of the triptych.⁴⁶ The title indicates that the print is based on a report in the June 10 edition of the *Tokyo nichinichi shinbun*, but in fact the text and the visual image only loosely follow the newspaper report. Okada's text describes the May 22 battle at Sekimon in the following way:

It was in the seventh year of Meiji [1874] when Japanese soldiers came to this island to punish the violence of the Taiwan raw savages. But the untutored barbarians do not know ethics and they attacked without warning, so at last the hideout of the Botan [Butan] race was attacked in May in order to suppress them with the military power [*hei'i*] of the empire [*kōkoku*] . . . [For their defense, the Butan] relied on cliffs so steep that a single person defending could impede the advance of ten thousand, and using large stones they made battlements that blocked any movement. From within this [stronghold] they let loose a hail of gunfire. At this point our troops contrived a plan. They managed with great effort to circle around a mountain that had no footpaths, and aiming down at the stronghold of the Botan from the side of the mountain they fired, [their gunfire] like hail pelting down in an onslaught by a strong mountain storm. The savages lost heart and they surrendered and apologized, and at dawn on May 22 Japan's imperial prestige [*ten'i*] shone before the world [*bankoku*] in this battle at Sekimon.

Okada credits the victory over the aborigines to Japan's military power, evoking the prestige of the samurai class, but according to Okada, prestige for the victory also accrues to the empire, and the most important result is to make Japan's imperial prestige shine throughout the world. While Okada's rhetoric leaves open the question of whether imperial prestige or samurai valor constituted the source of Japanese dominance over the aborigines, his text conveys an unmistakable message about how the instrumental use of dominance overseas could improve Japan's status in the world.

The visual images in the print use a different mixture of codes to convey Japanese dominance over the aborigines, and they convey a different ambiguity about the source of Japanese dominance, suggesting that it may derive either from Japan's martial tradition or from its engagement with Western civilization. Read from upper right to lower left, the images in the print convey Japanese dominance in multiple ways. The Japanese figures are placed higher in the print, they appear either composed and confident (such as the officers on the right) or fierce and threatening (such as the soldiers in the middle), and they stand over the dead bodies of several aborigines (the second Japanese figure from the right, for example, holds up the head of a dead aborigine by his queue). The portrayal of the aborigines, on the other hand, evinces an utter lack of valor. They are either dead (foreground) or running away from the Japanese in fear (background). The depiction of Japan's military victory draws on the enormously popular theme of samurai valor from Edo-period woodblock prints, but at the same time the tradition of samurai valor has been clothed quite literally in Eurocentric symbols of power characteristic of

⁴⁶ The signature in the cartouche at the far left identifies the author as Onkatsu Ryūgin "Okada," a penname of Okada Jisuke. Miyatake Gaikotsu and Nishida Taketoshi, *Meiji shinbun zasshi kankeisha ryakuden*, vol. 20 of *Meiji Taishō genron shiryō* (Tokyo, 1985), 34. I would like to thank Henry Smith for bringing this fact to my attention.

the *bunmei kaika*: the soldiers sport, for example, close-cropped hair, trousers, and jackets. Little more than a decade earlier, it would have been unimaginable for samurai to wear such clothing, and even when a few samurai broke with tradition during the waning years of the old regime to experiment with the new Western garb, they faced the prospect of punishment.⁴⁷ To be sure, official attitudes toward Western clothing changed during the early years of the Meiji period, but the adoption of Western clothing—and, more broadly, Western material culture—became a site of bitter contention as some Japanese vigorously opposed any Western influence on Japan, and this was particularly true among those members of the samurai class who were inclined to rebel against the fragile Meiji government. The European aspect of the soldiers' appearance in Yoshiiku's print may have provided a general idea of how members of the expeditionary force actually looked, but their appearance also surely evoked an awareness of samurai resistance to Western influence in Japan. Yoshiiku's portrayal of the soldiers seems to betray an awareness of this resistance, because he chose not to portray rifles, a weapon that true samurai scorned as beneath their dignity and that some samurai saw as a Western perversion of Japan's purity. His choice seems all the more deliberate since newspaper articles—and even the text by Okada that appears on the print—highlighted the Japanese use of rifles, and since his greatest rival at the time, the woodblock print artist Tsukioka Yoshitoshi, accentuated the Japanese use of rifles in his print about the battle at Sekimon.⁴⁸ Yoshiiku's easy blending of samurai valor and the appropriation of Eurocentric civilization thus elided the hotly contested issue of whether it was proper to introduce Western civilization into Japan, and it clouded the question of whether Japan's victory over the aborigines occurred because of Japan's tradition of samurai valor or because Japan had appropriated attributes of Western civilization.

The combined visual and verbal depiction of the battle at Sekimon by Yoshiiku and Okada offers a contradictory view of Japanese dominance, as do other commercial sources that will be discussed in the next section of this essay. The crossed codes of dominance betray a complex and ambivalent appreciation of dominance that arose because of contention over how Japan should engage Western civilization and what the new basis of political authority ought to be. In the 1870s, then, Japan's mimetic imperialism did not rely on a consensus about how Japan should engage Western civilization, or what political authority meant. Commercial sources do suggest, on the other hand, that mimetic imperialism entailed accepting the instrumental use of force as a way of improving Japan's position in the world, and that establishing dominance over the aborigines took place in the context of appropriating Western civilization and redefining the nature of political authority in Japan.

REPRESENTATIONS OF THE SUBMISSION of the aborigines similarly asserted a higher status for Japan while evoking the troubled and contradictory terms on which Japan

⁴⁷ Eskildsen, "Telling Differences," 331.

⁴⁸ Cited in n. 42 above.

engaged Western civilization. In June and July of 1874, Japanese newspapers reported how the leaders of various villages in southern Taiwan had capitulated to Japanese authority, which they described as an act of submission (*kijun*). As they did with their expressions of Japanese dominance, commercial sources about the Taiwan Expedition refracted their representations of the submission of the aborigines through an understanding of civilization and savagery, and they conveyed a number of messages about the nature and purposes of the expedition that differed from the government view. Most important, the government saw submission as a step toward establishing Japanese jurisdiction over the aboriginal territory, which would justify the Japanese effort to annex it. Commercial sources, on the other hand, generally interpreted the submission of the aborigines as part of a process of establishing Japanese authority over them. While the explanations of submission diverged, the effects they envisioned were similar: in either case, establishing jurisdiction or authority served as a necessary first step in the process of bringing civilization to the savages. In the case of commercial sources, however, portraying the submission of the aborigines also involved a key modification of what submission meant in order to distinguish Japan's encounter with Western imperialism from the Taiwanese aborigines' encounter with Japan's mimetic imperialism. Modifying what submission meant, in other words, helped draw attention away from the similarities between Japan's subordination to the West and the aborigines' subordination to Japan.

As soon as word of the expedition's activities on Taiwan reached Japan, Japanese newspapers began to carry accounts of how the aborigines had surrendered to Japanese forces. On June 15, both the *Yūbin hōchi shinbun* and the *Kōbun tsūshi* published an account, based on a letter from a member of the expedition to the branch of the Bureau of Savage Affairs in Nagasaki, that explained how the chiefs of seven aborigine villages in southern Taiwan came to the Japanese camp on May 24 to capitulate.⁴⁹ According to the account, the chiefs received gifts of blankets, swords, and Japanese flags as a reward for their cooperation in the fight against the Butan, and the Japanese and the chiefs sealed their friendship with toasts of champagne and beer. Over the ensuing weeks, more villages capitulated, and newspapers reported that by the end of July all of the villages of southern Taiwan had submitted to the Japanese. Most newspaper accounts described the capitulation of the aborigines in formulaic terms, usually including how the aborigines exchanged items of local produce for Japanese gifts. Exchanges of this sort resemble the ritual exchange of gifts that took place under Chinese-style diplomacy, and, while the exchanges implied a strict subordination of the aborigines to Japanese authority, they did not necessarily involve an assertion of Japanese jurisdiction. The role of flags in the exchanges with the aborigines does reveal, however, an attempt by members of the expedition to assert jurisdiction over southern Taiwan.

A careful look at the exchange that took place on May 24 suggests that the aborigines understood the Japanese flags they received as symbols of protection. On June 23, the *Japan Daily Herald* published a long account of the events on

⁴⁹ *YHS*, June 15; *KT*, June 15. Most sources agree that the meeting took place on May 24, but these newspaper reports list the date as May 25.

Taiwan, probably based on reports from the American newspaper correspondent Edward House, who had accompanied the expedition to Taiwan.⁵⁰ It included a description of the negotiations led by Douglas Cassel, an American military adviser to the Japanese who handled the negotiations with the aborigines. Cassel drew an enthusiastic response from the assembled chiefs when he promised on behalf of the Japanese that each village that pledged its cooperation would be given a Japanese flag guaranteeing its protection. As obvious as the symbolism might appear to us today, the aborigines would not have understood the flags as a national symbol. Rather, they would have seen them as a signal of cooperation and non-aggression similar to an arrangement LeGendre made with them in 1867, where pieces of red cloth—a luxury item to the aborigines—symbolized the agreement.⁵¹ Cassel's description of the negotiations suggests that the aborigines viewed the 1874 agreement with the Japanese as a quid pro quo arrangement of cooperation in exchange for a guarantee that the Japanese would not attack them, a reassurance they wanted in the wake of the devastating Japanese attack on the Butan at Sekimon only a few days earlier. According to Cassel, the decision to give each chief a Japanese flag as a sign of protection was made after the leader of the aborigines took the initiative and asked the Japanese for papers that would guarantee their protection, similar to a written agreement LeGendre had made with them several years earlier. The aborigine leader requested the guarantee of Japanese non-aggression, though, just after he guaranteed the Japanese that they could land their ships and take on supplies in the coastal areas of southern Taiwan.⁵² It seems likely, then, that the aborigines understood the two guarantees as an exchange.

The Japanese, on the other hand, ascribed a more far-reaching colonialist meaning to the flags: they saw them as a symbol of jurisdiction over the aborigines. Between the end of May and the end of September, the Japanese occupation force distributed flags and “certificates of submission” to over fifty villages throughout southern Taiwan, including villages well outside the area where the fighting took place. On the front, the certificates bore the seal of the government-general (*totoku-fu*) as well as the name of the chief receiving the certificate and the name of his village; on the back, according to one template, they read:

The person who bears this certificate has submitted [*kijun*] to the empire, thus he should not be treated with violence.

Meiji 7, sixth month.

Headquarters of the army of Great Japan.⁵³

An account of the expedition by Mizuno Jun, one of the Japanese participants in the expedition, spells out the intended use of the flags and certificates. In his unpublished memoirs, Mizuno explained how people from villages all over southern Taiwan came to the Japanese camp and asked to become “good people under the jurisdiction of the Japanese government” (*Nihon seifu chika no ryōmin*), in return

⁵⁰ JDH, June 23. For House's account of the expedition, see Edward Howard House, *The Japanese Expedition to Formosa* (Tokio [Tokyo], 1875).

⁵¹ LeGendre, “Notes of Travel,” 3: 241–43, 282–83, LeGendre Papers.

⁵² Cassel to LeGendre, May 26, 1874, “Okuma monjo” C453.

⁵³ Ochiai, *Meiji shichinen seiban tōbatsu kaikoroku*, 104.

for which they received the flags and certificates.⁵⁴ According to Mizuno, the villagers were instructed to fly the flags if Japanese troops passed through their area, or to show the certificates as a sign of their allegiance to the Japanese government (*Nihon seifu ni reizoku suru hyōshō*) if they came to the Japanese camp. The phrase “good people” often appears in Chinese sources, and the aborigines appear to have used it to distinguish themselves from troublemakers who deserved to be punished, but Mizuno adds to this conventional Chinese locution the colonialist logic of the Japanese government when he invokes the idea of jurisdiction. Mizuno’s description of the flags provides firm evidence that members of the expeditionary force saw their actions as an attempt to establish Japanese jurisdiction, which, according to the official rationale for the expedition, provided a legal basis for colonization.

In contrast to the view of submission held by members of the expedition, commercial sources rarely depicted the submission of the aborigines in terms of an assertion of Japanese jurisdiction. Instead, they usually linked submission to the assertion of Japanese authority and to the inauguration of a civilizing process among the aborigines. Moreover, in order to explain what submission meant, commercial sources often relied on a synthesis of symbols of hierarchy from the Edo period and symbols of dominance and subordination from “Civilization and Enlightenment” (*bunmei kaika*). The print shown in Figure 4, for example, employs a complex synthesis of symbols to convey a particular message about submission and the civilizing process. The print, by Yoshiiku, depicts a pair of aborigines bowing submissively before a fierce-looking Japanese soldier. Their respective postures express dominance and subordination in much the same way that Edo-period prints expressed social and political hierarchy, but the soldier’s cropped hair alludes to the civilizing imperative of the *bunmei kaika* and serves as a Eurocentric counterpoint to the Edo-period imagery that also defines his power. The soldier’s hairstyle and clothing, though not his weapon, serve as distinct and obvious markers of Western influence on Japanese culture. (For a contrasting view of samurai clothing and hairstyles from the 1850s, see Figure 7.) The aborigines thus bow down not to an undifferentiated image of Japanese authority but rather to an image of Japanese authority that was in the process of being transformed—sometimes in unwelcome ways—by Japan’s engagement with Western civilization.

The text, by Takabatake Ransen,⁵⁵ describes the process of subjugating the aborigines in somewhat different terms, building an implicit contrast between Japan’s engagement with Western civilization—“Civilization and Enlightenment”—and the enlightenment (*kaika*) of the aborigines:

All the savages of Taiwan have surrendered to our forces, but among them the savages of only the Botan [Butan] tribe fled deep into the mountains and did not come out, [so] the entire army attacked in mass from three directions, setting fire to the mountains so that they had no place to hide. The chiefs who had already submitted [*kijun*] acted as intermediaries,

⁵⁴ See Mizuno Jun, *Taiwan seibanki*, 4 vols., unpublished mss, 1879, vol. 4, no. 12, National Central Library, Taipei. Mizuno’s account of the flags is reproduced in Tairōkai, ed., *Tairō Mizuno Jun sensei* (Taipei, 1930), 287–96.

⁵⁵ The author of the text is Dendendō Dondon, penname for Takabatake Ransen, another of Yoshiiku’s associates at the *Tokyo nichinichi shinbun*. Okitsu Kaname, *Meiji shinbun kotohajime: “Bunmei kaika” no jōnarizumu* (Tokyo, 1997), 56–57.



FIGURE 4: Ochiai Yoshiiku (October 1874), "Tokyo nichinichi shinbun nanakyaku gojūni gō" (Tokyo Daily Newspaper, No. 752). Special Collections Room, Waseda University Library.

and on the first day of the seventh month of the year 2534 of imperial rule [*kigen*] [1874], [the Butan] came to the headquarters to apologize for [their] transgressions, and they surrendered in earnest. Thereafter the savage land became completely tranquil. It must be said that this expedition to punish the savages is the first stage in advancing the enlightenment [*kaika*] of this island.

As in other examples that have been discussed above, the text juxtaposes Japan's military prowess and its imperial authority, evoking the unsettled nature of domestic political authority. Specifically, the print contrasts a description of the military victory over the aborigines to the theme of imperial authority. It invokes imperial authority in this case by referring to *kigen*, a way of reckoning time based on the mythical origins of the imperial house that gained popularity in the Meiji period as an alternative to the Western reckoning of time, which assumed the primacy of Christianity. The text thus gave credit for the aborigines' submission to Japan's military prowess, it situated their submission in imperial time, and it identified their submission as the point at which the civilizing process could begin.

While the print represented the submission of the aborigines through a synthesis of allusions to power and hierarchy from the Edo period and to allusions to the *bunmei kaika*, it did so without establishing a basis of solidarity between the aborigines and the Japanese despite the fact that both of them were, after all, presumably going through their respective "enlightenments" at the same time. The print denies the possibility of solidarity between the Japanese and the aborigines, as its unmistakable message about hierarchy makes clear, and in doing so it implies that their respective enlightenments were incommensurate.

Other prints conveyed a similar message about the expedition, but they used different strategies to do so. For example, the print shown in Figure 5, the first panel of a pair of anonymous *kawaraban* entitled "Taiwan gunki" (A Military Tale of Taiwan), shows a striking image of aborigine submission that recalls some of the representations of the Perry Expedition from twenty years earlier, one of which will be described below. It would be difficult to exaggerate the importance of this expedition as a symbol of Western intervention in Japan. The Perry Expedition, which succeeded in inaugurating Western diplomatic relations in Japan, provoked an unprecedented wave of woodblock prints about the Americans and their ships,⁵⁶ and, a century and a half later, Perry's "black ships" still remain an important symbol of Japan's nineteenth-century encounter with the West. It should not surprise us, then, that in 1874 people in Japan saw the Perry Expedition as a paradigm of Western intervention and domination and attempted to explain Japan's involvement in Taiwan by analogy to it. In its explanation of aborigine submission, however, the print also relies on an extraordinarily dense web of analogies and allusions that touch on issues of Japan's national prestige, tropes of authority from the Edo period, and references to the *bunmei kaika*. Through its

⁵⁶ Tokyo Daigaku Shiryō Hensanjo Shozoku Gazō Shiryō Kaiseki Sentā, ed., *Surimono sōgō hennen mokuroku (kō)* (Tokyo, 1998). This chronological catalog lists extant monochrome prints (*kawaraban*) from the nineteenth century held in major Japanese collections. The list is comprehensive, but it understates the number of prints actually published (it does not list multicolored prints, prints in smaller collections, or prints that are no longer extant). The section on political and social disturbances lists the titles of approximately 250 different prints published in 1853–1854 that dealt with the Perry Expedition, American ships, or fortifications of the coast in response to the expedition (pp. 43–51).



FIGURE 5: Anonymous *kawaraban*, or monochrome print (c. 1874), "Taiwan gunki" (A Military Tale of Taiwan), panel 1 of 2. Special Collections Room, Waseda University Library.

various analogies and allusions, the print suggests that Japan's and the aborigines' experiences with imperialism were incommensurate.

To begin with, "Taiwan gunki" makes sense of the aborigines' submission through a synthesis, as did Yoshiiku's print. It portrays the expedition as a conflict between Japan as a nation and the aborigines as a savage people, and it defines Japanese authority through a mixture of Edo-period tropes and tropes from the *bunmei kaika*. For example, the visual image stresses the nationality of the soldiers by placing them beneath a banner that reads "Great Japan" and next to a pair of Rising Sun flags, symbols that had long histories in Japan but that became explicit symbols of the nation only in the Meiji period. The text, by contrast, stresses the savagery of the aborigines. The Butan, it says, "burrow into crags to make their dwellings, the spirit of the people is savage [*bōaku*] and they eat the flesh of people," combining the themes, described above, of cannibalism, cave dwelling, and savagery. The image of the aborigines on the left of the print shows them as large-headed and hirsute, perhaps making a visual analogy to the Ainu, who were usually described as a hairy people.

The image of the Japanese soldiers on the right offers a different synthesis, however: it is an odd conflation of Edo-period tropes of military authority and tropes of Western civilization from the *bunmei kaika*. The general, for example, sits



FIGURE 6: Shōchō Ikkei (1873), “Etoki gojūyokajō” (Pictorial Explanation of the Fifty-or-so Clauses), [clauses 1 and 2]. National Diet Library.

with his legs apart, a trope used in Edo-period prints of samurai to identify a military leader, and he wears a samurai's coat of arms and breastplate, attire abandoned during the modernization of the military during the early Meiji period. In Edo-period prints, military leaders would be shown sitting on folding stools, a sign of their authority over the lower-ranking samurai around them, but in this case the general is seated on a Western-style chair. Yet his chair is not simply a Western implement: in this context, it is a Western implement that has been appropriated to convey a particular message about authority. The use of chairs as signs of political authority can be seen, for example, in many illustrations of civil ordinances promulgated in Japan only a year or two before the Taiwan Expedition as part of a self-conscious effort to “civilize” Japan.⁵⁷ Illustrations of the ordinances regularly depicted government officials seated in chairs, clad in Western-style trousers, and sporting close-cropped Western-style haircuts, as they dispensed justice to the Japanese transgressors who knelt before them in clothing and hairstyles characteristic of the Edo period (see Figure 6). The chair in “Taiwan gunki” thus refers not to Western civilization but rather to the adaptation of Western civilization as part of an effort of redefine political authority in Japan, and it shows how it was possible to imagine that Japan's new, partially Westernized political authority could be

⁵⁷ Tokyo-to, ed., “Ishiki kaii jōrei o shikōsu,” *Tokyo shi shikō (shigai hen)*, part 3, vol. 53 (Tokyo, 1962), 691–709; Kumakura Isao, “Bunmei kaika to fūzoku,” in Hayashiya Tatsusaburō, ed., *Bunmei kaika no kenkyū* (Tokyo, 1979), 576–78; Eskildsen, “Telling Differences,” 366–74.

exerted overseas. The cropped hair and clothing of the soldiers surrounding the general in “Taiwan gunki” make a similar statement about the relationship between the changing nature of domestic political authority and the *bunmei kaika*.

More striking, however, is the visual allusion to the Perry Expedition that “Taiwan gunki” makes. The composition of the visual image mimics prints that depicted the fictive submission of Commodore Perry and the American “barbarians” in 1853–1854 (see Figure 7).⁵⁸ In spite of the government’s utter failure to expel the Perry Expedition, dozens of the most popular prints from those years inverted the foreign threat and showed Perry or Americans as a defeated enemy.⁵⁹ In “Taiwan gunki,” as in prints of a “defeated” Perry in the 1850s, the dominant Japanese forces sit beneath military banners, and their leader faces the submissive enemy magisterially, while the enemy bows down in obeisance to the Japanese. Through this visual allusion to prints of a defeated Perry, “Taiwan gunki” inscribed the Western imperialist power to dominate onto Japan, in effect replacing an older conception of Japanese superiority with a new one that was defined partly by reference to the attributes of Western civilization, and it displaced the subordination of the American “barbarians” onto the aborigines.

Inscribing the power of Western imperialism onto Japan presented an opportunity to assert for Japan a higher status in the world, and the text attends to this when it voices concern about Japan’s prestige. It invents two speeches by Japanese leaders who warn against bringing shame upon Great Japan, and it claims in its conclusion that the military campaign that forced the last of the aborigines to submit “is Japan’s glory and is truly a courageous feat.” Displacing the “barbarism” of the Americans onto the aborigines, on the other hand, entailed a complementary transformation of what subordination meant. For one thing, it straightened out the inverted view of submission in prints from the 1850s—Perry never capitulated in the 1850s, whereas the aborigines actually did in 1874. For another, the text defined the subordinate status of the aborigines through the Western dichotomy of civilization and savagery rather than through Edo-period categories of barbarism that were used to describe Perry and the Americans in the 1850s. To understand the print’s displacement of subordination thus required conceiving of Japan’s relationship to the aborigines through a dense and complex set of inversions and analogies, but the meaning of the displacement was readily apparent: it showed how Japan’s subordination to the West could be displaced onto the aborigines. In this example, too, the portrayal of the aborigines’ subordination foreclosed the possibility of solidarity between the Japanese and the aborigines in favor of asserting a hierarchical relationship between them patterned in part after Western imperialism.

Commercial representations of the submission of the aborigines thus show a

⁵⁸ Eskildsen, “Telling Differences,” 166–71, 178–201.

⁵⁹ Prints from 1853–1854 showed the “defeat” of the Americans by analogy to the defeat of the Mongol invasions in the thirteenth century, as well as by representing Americans who were (fictively) captured in battle, overpowered by Japanese military might, overpowered by Japanese gods, or, as in Figure 7, cowering before Japan’s great leader. At least two other compositions from 1853–1854 use exactly the same image of Perry cowering before the Japanese that is shown in Figure 7. For a discussion of inverted views of the foreign threat in 1853–1854, see Eskildsen, “Telling Differences,” 178–201.



FIGURE 7: Anonymous and untitled *kawaraban* (c. 1854), showing Commodore Perry capitulating to the Japanese. Kurofunekan, Kashiwazaki City, Niigata Prefecture.

keen awareness of how Western civilization could be adapted to describe new hierarchical relationships that resembled the relationships of Western imperialism. Members of the expeditionary force, pursuing the colonialist logic of the government, interpreted the submission of the aborigines as an assertion of jurisdiction, a legal precondition that would make it possible to claim the aboriginal territory for Japan. Commercial sources, on the other hand, interpreted submission more broadly, most often seeing it as an assertion of Japanese authority. Irrespective of the government's motives, but by no means antithetical to them, commercial sources depicted the submission of the aborigines both in terms of the contested nature of domestic political authority in Japan and in terms of Japan's engagement with Western civilization. To be sure, an assertion of Japanese authority over the aborigines made Japan look more civilized than a mere assertion of jurisdiction would, but it did more than this. It also made it possible to assert that Japan had begun the process of civilizing the aborigines, a key point of congruence between the government's goals for the expedition and the expectations voiced in commercial sources. At the same time, the submission of the aborigines to Japanese authority exaggerated the contrast between the respective civilizing processes that presumably were taking place simultaneously in Japan and among the aborigines, cutting off the possibility of a sympathetic comparison between their experiences. Further, one of the prints described above achieved the same effect by portraying a symbolic displacement of Japan's subordinate status onto the aborigines. In sum, commercial sources about the expedition depicted the theme of the submission of the aborigines to Japanese authority in a variety of ways that asserted a higher

status for Japan while foreclosing the possibility of solidarity with the aborigines, and they did so partly by reference to Japan's engagement with Western civilization and partly by reference to older conceptions of hierarchy that helped explain a new relationship between Japan and Taiwan that mimicked Western imperialism.

THE CULTURAL DIMENSION of Japan's mimetic imperialism stands out when we examine commercial sources that describe the Taiwan Expedition. To be sure, government documents show how the Japanese government received a particular set of ideas about civilization and savagery and used them to justify an attempt to establish colonies on Taiwan, but the government view can take our understanding of the expedition only so far. In order to ground our understanding of the expedition more fully in the historical context of Japan during the 1870s, and to gain a fuller understanding of the expedition's mimetic imperialism, we need to broaden our perspective to include commercial sources. The most striking insight produced by this approach is the recognition that the reproduction of Western imperialism in Japan took place in the context, and as part of the process, of Japan's appropriation and adaptation of Western civilization. One implication of this insight is that in order to understand the mimetic imperialism of the expedition, and perhaps to understand the origins of Japanese imperialism more generally, we need to look beyond diplomatic, economic, and strategic motivations for imperialism and recognize that Japan's aggressive stance toward East Asia constituted an integral part of its contested engagement with the Western civilization.

In order to elucidate this cultural connection, the foregoing discussion has emphasized several ways that commercial sources adapted Western thinking about civilization and savagery. First, the sources exaggerated the cultural difference between the Japanese and the aborigines of Taiwan by accentuating their savagery. Second, they used analogies to Edo-period conceptions of hierarchy in order to explain the hierarchical relationship that the expedition intended to establish between the "civilized" Japanese and the "savage" Taiwanese. Third, they framed that hierarchical relationship in terms of Japan's domestic political authority, which remained confused in the wake of the Meiji Restoration. Finally, they explained the hierarchical relationship partly by reference to Japan's own troubled encounter with Western civilization and Western imperialism.

Commercial sources do not explain why they adapted the ideas of civilization and savagery this way, but by examining the effects of the adaptations we can make some inferences about what their purposes may have been. First, and most obviously, the adaptations made it easier for people in Japan to claim for their nation a higher status in a world hierarchy defined and dominated by the West. Japanese people could, in other words, claim a higher status for their nation by modifying what civilization and savagery meant. Because Japan did not belong to the West, however, appropriating attributes of Western civilization could not produce a simple or straightforward increase in Japan's status. Claiming higher status for Japan on Western terms threatened claims to power that relied on indigenous attributes, such as samurai authority and imperial divinity, and the

threat posed by appropriating Western civilization stimulated debates about the nature of political authority in Japan throughout the 1870s and beyond. A second effect, similar to Fukuzawa Yukichi's call to "escape Asia," involved evacuating a middle ground between civilization and savagery that might have served as a basis of solidarity between the Japanese and other peoples of East Asia. Removing this middle ground did more than simply deny the possibility of solidarity, though; it also asserted aggressively that Japan could, by appropriating Western attributes of dominance, displace its own Western-imposed subordination onto other peoples in East Asia. A denial of solidarity meant, in other words, not simply an escape from Asia but also a concomitant assertion of dominance over others in Asia. Third, the adaptations made it easier to see Japanese dominance over a foreign people through the lens of the ongoing debate about the contested and ambiguous nature of domestic political authority. Thinking about what civilization and savagery meant in the case of the Taiwan Expedition offered an opportunity, in other words, to think about what imperial authority might or might not mean, or how the attributes of Western civilization might or might not be suitable for Japanese society.

Finally, the timing of the expedition's mimetic imperialism is crucial because it shows that mimetic imperialism occurred as part of the process, not the result, of Japan's engaging Western civilization. The timing shows that, from the outset of the Meiji period, the cultural process through which people in Japan engaged Western civilization had an important influence on how they viewed their nation's role in the international order, and that mimetic imperialism played a part in how domestic political power and national identity were redefined in the 1870s. Commercial representations of the Taiwan Expedition therefore implicate mimetic imperialism in Japan's broader adaptive response to Western civilization and in the effort to refashion Japan into a modern nation-state.

Robert Eskildsen is an assistant professor of Japanese history at Smith College. His dissertation (Stanford University, 1998) examined the ideology of visual representations of foreigners in nineteenth-century Japan, and the present article grew out of a brief discussion in the dissertation of a few seemingly minor woodblock prints that depicted the aborigines of southern Taiwan. Eskildsen is currently writing a book that will explain the cultural and diplomatic history of the Taiwan Expedition.

Asia and Europe in the World Economy: Introduction

PATRICK MANNING

THE ARTICLES IN THIS FORUM reinforce one another in emphasizing three fundamental points: East Asian economies maintained strength and vitality up to the end of the eighteenth century, they participated in an interconnected world economy throughout the early modern era, and only sometime after 1800 did they fall behind rapidly growing European economies. Each of these points has been argued in earlier writings, but their combination is an innovation. Kenneth Pomeranz and R. Bin Wong, in combining these points, elaborate an approach to the world economy that is global rather than piecemeal. Their approach goes beyond comparison of discrete regional economies and beyond explaining the world economy in terms of dominance by a single economic pole. Instead, they focus on the interaction of at least two economic poles—Western Europe and China—and more generally on the connections among regions of the world.

The principal implication of these studies is that the great shift in the balance of the world economy—the establishment of European economic hegemony—took place rather late, in the age of steam-powered industrialization, long after the sixteenth-century era of initial European maritime expansion. In developing a new argument for a late turning point in world economic history, the participants in this *Forum* confirm a turning point in the historiography of the global economy. That is, they have elaborated a globalized—rather than localized or comparative—line of argument on the world economy.

While the notion of the world economy is widely recognized, it has usually been put into practice as an accumulation of discrete national units for which data are available, rather than as an aggregated macroeconomic system. For this reason, neither the totality of the system nor the interactions among its parts have been given much emphasis. Instead, scholars have explored the world economy by studying region after region, to determine which was dominant.¹ The present writers, in company with other collaborators, have taken the further step of suggesting linkages among regional economies that begin to focus not so much on which segment dominated as on how the segments fit in the functioning of a larger, planetary economy.

¹ Douglass C. North and Robert Paul Thomas, *Rise of the Western World: A New Economic History* (Cambridge, 1973); Nathan Rosenberg and L. E. Birdzell, Jr., *How the West Grew Rich: The Economic Transformation of the Industrial World* (New York, 1986); William N. Parker, *Europe, America, and the Wider World*, Vol. 1: *Europe and the World Economy* (Cambridge, 1984); David S. Landes, *The Wealth and Poverty of Nations: Why Some Are So Rich and Some So Poor* (New York, 1998).

Pomeranz and Wong use distinct yet complementary approaches. They work, one may argue, from a common fund of materials and approaches: comparisons, linkages, new data, and a global paradigm. In comparisons among regions, they explore new data and match up various rates of economic growth, and levels of output, consumption, and trade. In linkages, they pursue commercial flows from region to region. The linkages are a rather new approach, made possible by growing funds of data for regions outside Europe.

The assumption of a global economy enables these straightforward techniques and the new data to produce a substantially new interpretation. Pomeranz and Wong assume that the world economy consisted of a very large, multipolar system. In so doing, they depart from earlier frameworks, which have tended to assume the existence of distinct, discrete, regional economic systems ("Europe," "China," "India," and perhaps "Africa," for instance), or which have assumed the existence of a single world economic system, centered in Europe, which gradually expanded its influence.²

The difference is not so much that these authors have proved the existence of a global economy for which others failed to see the evidence. Previous authors have worked within a paradigm assuming a set of distinct regional or civilizational economies—a paradigm that did not encourage them or perhaps did not allow them to emphasize the overarching and interconnecting trends in the economy, until the era of unmistakable European hegemony from the mid-nineteenth century. Thus the data on world silver trade, while central to this discussion, are not entirely new. Rather, the point is that Dennis Flynn and Arturo Giráldez, in their studies of the silver trade, have shown its significance in elucidating a picture of the global economy. Their affirmation that over half the world's silver extracted between 1600 and 1800 ended up in China, where it served especially as specie for payment of taxes, has served to link the global pieces, so that what was long a debate on isolated comparisons has become a debate on interaction.³ These authors, along with others in what has been called a "California school" because of their residence, are creating a historiographical turning point. Andre Gunder Frank, in linking many of these contributions in his book *ReOrient*, articulated a sweeping general statement of the case for an Asian-focused world economy and the late achievement of European economic hegemony.⁴

² For approaches emphasizing discrete regions in the world economy, see Paul Bairoch, *The Economic Development of the Third World since 1900*, Cynthia Postan, trans. (Berkeley, Calif., 1975); and E. L. Jones, *The European Miracle: Environments, Economies, and Geopolitics in the History of Europe and Asia* (Cambridge, 1981). For approaches emphasizing incorporation of outlying regions, see Immanuel Wallerstein, *The Modern World-System*, vol. 1 (New York, 1974); and Eric R. Wolf, *Europe and the People without History* (Berkeley, 1982).

³ Dennis Flynn and Arturo Giráldez, "Born with a 'Silver Spoon': The Origin of World Trade," *Journal of World History* 6, no. 2 (1995): 201–22; Flynn and Giráldez, "Arbitrage, China, and World Trade in the Early Modern World," *Journal of the Economic and Social History of the Orient* 38, no. 4 (1995): 429–48. Rene J. Barendse has intervened in this discussion to argue that the portion of global silver trade going to India in the seventeenth century was larger than that estimated by Flynn and Giráldez. Barendse, *The Arabian Seas: The Indian Ocean World of the Seventeenth Century* (Armonk, N.Y., 2001).

⁴ What Frank has called the "California school" includes Pomeranz and Wong (both at University of California, Irvine), Flynn and Giráldez (both at University of the Pacific), Jack A. Goldstone (University of California, Davis), John E. Wills (University of Southern California), and Richard von Glahn (UCLA). See Jack A. Goldstone, *Revolution and Rebellion in the Early Modern World* (Berkeley,

Frank's *ReOrient* appeared in 1998. The nearly simultaneous publication of David Landes's *Wealth and Poverty of Nations*, which restated the case for European dominance of the world economy from the voyages of discovery, created the opportunity for a marvelously lively electronic debate, testing the two viewpoints.⁵ In that debate and in subsequent discourse, participants have had to consider at once the European and East Asian poles of the world economy.

The present articles carry forth the articulation of this substantially shifted paradigm with the addition of new empirical data, presented in a variety of cross-regional comparisons. Some time-honored techniques of analysis, when applied in combination with new data and with a framework sensitive to global interactions, have yielded a set of challenging hypotheses on the operation of the world economy in the eighteenth century.

What has caused the paradigm to shift? Paradigm shifts, whether described in Kuhnian terms or more generally, are the points at which quantitative changes lead to qualitative leaps in conceptualization.⁶ This paradigm shift emerges from the confluence of several distinct yet overlapping trends in scholarship and public discourse—trends that progressively undermined the view of the world economy as a cumulation of local economies, replacing it with the model of a global system. First is the continued expansion of research in economic history. Although economic historians have lost the prominence they gained in their early applications of quantitative and theoretical analysis, especially in the 1970s, their work since has advanced the conceptual rigor and unity in historical studies of economic life. The development of comparable data for the economies of region after region may permit a return of economic issues to the forefront of historical debate, now at a global rather than national level.⁷ Secondly, the expansion of teaching and research in world history has led to the development of global and interactive modes of thinking in historical context.⁸ Thirdly, the recent economic boom and bust of East

Calif., 1991); John E. Wills, Jr., "Maritime Asia, 1500–1800: The Interactive Emergence of European Domination," *AHR* 98 (February 1993): 83–105; and Richard von Glahn, *Fountain of Fortune: Money and Monetary Policy in China, 1000–1700* (Berkeley, 1996).

⁵ Andre Gunder Frank, *ReOrient: Global Economy in the Asian Age* (Berkeley, Calif., 1998); Landes, *Wealth and Poverty of Nations*. Frank's argument relied significantly on work by Wong and Pomeranz, now published as R. Bin Wong, *China Transformed: Historical Change and the Limits of European Experience* (Ithaca, N.Y., 1997); and Kenneth Pomeranz, *The Great Divergence: China, Europe, and the Making of the Modern World Economy* (Princeton, N.J., 2000). The electronic debate on the positions taken in the books of Frank and Landes took place from May 11 to June 9, 1998, and was shared among four electronic discussion lists: H-WORLD (world history: <http://www.h-net.msu.edu/~world>), H-Asia (Asian studies: www.h-net.msu.edu/~asia), EH.RES (Economic history research: <http://eh.net/lists/archives/eh.res/>), and WSN (World systems network: <http://csf.colorado.edu/wsystems/wsarch.html>). The electronic debate was followed, on December 2, 1998, with a debate between Frank and Landes at Northeastern University, for which the transcript is online at www.whc.neu.edu/whc/seminar/pastyears/frank-landes/.

⁶ Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 3d edn. (Chicago, 1996); Paul Horwich, ed., *World Changes: Thomas Kuhn and the Nature of Science* (Cambridge, Mass., 1993).

⁷ For an overview showing the progress of economic historians beyond national studies to a global interpretation (though for the period after 1850), see Jeffrey G. Williamson, "Globalization, Convergence, and History," *Journal of Economic History* 56, no. 2 (1996): 277–306. The recent study by Kevin H. O'Rourke and Jeffrey G. Williamson, *Globalization and History: The Evolution of a Nineteenth-Century Atlantic Economy* (Cambridge, Mass., 1999), while innovative in several dimensions, still relies on the comparison of national economic statistics.

⁸ John F. Richards, "Early Modern India and World History," *Journal of World History* 8, no. 2

and Southeast Asia has drawn popular and scholarly attention to that region and its place in the world economy. Fourthly, the emergence of new views of industrialization is drawing increasing attention both to the long-term growth and transformation of many regional economies and to the connections among regions that preceded industrialization and then deepened in the course of industrialization.⁹ Finally, within the expanding field of East Asian studies, scholars have turned in recent years from treating the region in isolation to seeking out links and similarities to other parts of the world.¹⁰ The interplay of these trends has led, rather suddenly, to a sharp expansion in the scale and interactive detail of interpretation in early modern economic history.

As for the details of their arguments, both Wong and Pomeranz adopt a standpoint in China, but from that standpoint they inquire into economic changes and continuities in several major regions. Kenneth Pomeranz addresses the issue with an empirical and comparative strategy centering on the eighteenth century, addressing consumption, production, labor, and giving particular attention to ecological constraints on growth. For Pomeranz, "political economy" means neoclassical microeconomic analysis, with attention to resource allocations, factor markets, product markets, and income levels. His argument centers first on comparisons between European and Chinese regions in levels of consumption (total caloric intake, and consumption of sugar, tea, cotton, and silk). Then he turns to labor markets, arguing that women's earnings were close to men's in China. On ecological limits to growth, he argues that deforestation and shortages of fuel and building materials were no more serious in China than in Europe. In his conclusion, Pomeranz argues that the big change was not a collapse in the Chinese economy but an acceleration of European economic growth, especially because of Britain's combined resources in iron and coal, plus the availability of resources from the Americas.

R. Bin Wong adopts a strategy of analytical comparison, contrasting the political economies of agrarian and merchant empires. For Wong, "political economy" means government economic policy and patterns of long-distance trade. He elucidates his argument with a counterfactual comparison, imagining that a South China leader might have seized control of the empire and led it under a mercantile policy.¹¹ He concludes that such an approach would not have led to substantial additional growth for China. Wong's focus on linkages includes emphasis on Japanese-Chinese ties: he argues that both economies grew through the eighteenth century as a result of their commerce. In comparing types of firms, Wong concludes

(1997): 197–210; Nicola di Cosmo, "State Formation and Periodization in Inner Asian History," *Journal of World History* 10, no. 1 (1999): 1–4; and Anthony Reid, *Southeast Asia in the Age of Commerce, 1450–1680*, 2 vols. (New Haven, Conn., 1988–93).

⁹ For recent trends in thought on industrialization, see E. A. Wrigley, *Continuity, Chance and Change: The Character of the Industrial Revolution in England* (Cambridge, 1988); and Jan de Vries, "The Industrial Revolution and the Industrious Revolution," *Journal of Economic History* 54, no. 2 (1994): 249–70.

¹⁰ Jonathan Lipman, *Familiar Strangers: A History of Muslims in Northwest China* (Seattle, 1998).

¹¹ Comparison of the historical record with a counterfactual yet logically significant alternative has been part of the formal toolkit of economic history since the work of Robert William Fogel, as presented especially in his *Railroads and American Economic Growth: Essays in Econometric History* (Baltimore, 1964).

that, in Asian markets, there was no single superior form of the firm, and thus the failure of Asian firms to adopt the European joint-stock model does not provide an adequate explanation of eventual European hegemony. In explaining this hegemony, Wong gives particular attention to the expanding opium trade as a device to eliminate silver flows to China. More generally, Wong argues that the world economic history of the nineteenth century was substantially different from that of the previous centuries, and that the notion of an “early modern” period is therefore rendered all the more valid and useful. His analysis, while centering on the eighteenth century, ventures back to the sixteenth century and forward to the turn of the twentieth century.

Both authors emphasize the range of regions that may be appropriately compared to each other, and conduct analyses in which the regions compared and connected are flexible in their extent. They include comparisons of East and West; of China and India and Europe; of Britain and France with Jiangnan (the lower Yangzi Valley) and Lingnan (the provinces surrounding Canton); and of regions as small as the British Midlands, Belgium, and the industrial centers of South China.

David Ludden, in his wide-ranging comment on the *Forum* articles, finds the economic arguments of Wong and Pomeranz to be convincing, summarizes parallel arguments for South Asia, and then provides two narratives linked to the implications of these articles. First is a narrative of perceptions of inequality among European writers of the nineteenth century, in which Ludden argues that Europeans began assuming their economic dominance before they could be sure it existed. Second is a narrative of successive regimes of economic inequality, from 1820 to the present, emphasizing the growing gap between the haves and the have-nots. Ludden’s commentary provides a foretaste of the restructured interpretation of the industrial era—and the critique of economic inequality—that is likely to result if the theses presented in this *Forum* come to be accepted.

Yet analytical paradigm shifts, however exciting they may be in retrospect, are rarely straightforward or coherent in practice. How global is the thinking in the current debate on the history of the global economy? Readers should, in my view, check carefully into their own assumptions and the assumptions of authors on what model of the early modern world economy is implied or assumed. Among those who agree that there existed a world economy for the eighteenth century, there may still exist several alternative visions of that world. Here is a reduction of the many possible models to four alternatives. One can readily see the differences between a scattered or kaleidoscopic model (in which a series of local economies are summed up into a global total) and a monopolar model (in which one great center dominates the planet). Between these extremes, one can also consider a bipolar model (with competing centers in Western Europe and China) and a multipolar or distributed model (in which several centers compete and contribute).

The consequences of these four models differ greatly. For a specialist in Western Africa such as myself, for instance, a multipolar model has distinct advantages over a bipolar Europe-China model. That is, I remain skeptical of models of the world economy that give no explicit place for Africa. West Africa was a region of socioeconomic vitality that suffered substantial losses through slave trade yet remained active in global commerce. Currently predominant views of the

world economy assume that Africans may safely be neglected, or they give consideration only to those Africans constrained to leave the continent. I seek a model of the world economy sufficiently nuanced to highlight the give-and-take in economic relations among regions, not just focus on centers of dominance. Pomeranz and Wong here concentrate on presenting comparisons that convey a view of the interactions between East Asia and the rest of the world economy, and in so doing they take a crucial step forward. The next step is up to readers as much as to these authors. Do these challenging comparisons imply that the view of a monopolar, Euro-dominant world economy should be replaced by that of a bipolar world economy? Ludden's emphasis on South Asian examples of global economic interaction, in his commentary, may reflect a concern with just this point. Further development of our understanding of the early modern world economy should explicitly identify whether the framework is an assumed monopolar economy, a bipolar economic system, or a multipolar world economy relying on systemic interplay including the Americas, West Asia, South Asia, Africa, Russia, and beyond.

Ultimately most important in these economic historical analyses are the data. In pursuing the details of regional comparisons, it is difficult to establish the equivalencies between prices, values, and rates of the different regions, for instance in comparing the caloric intake of people in the Yangzi Valley and in Britain during the eighteenth century. Yet in addressing the difficulties of today's interregional comparisons of historical data, one may take inspiration from the accomplishments of those who actually conducted the world trade of the eighteenth century. For those merchants and consumers, assessing the value of silks, woolens, and tea after thousands of miles of voyages across currency zones was both laborious and approximate. But somehow the trade of the eighteenth century was carried on, despite such difficulties in establishing the equivalencies. For the present debate to be empirical, not just conceptual or ideological, we need to continue making comparisons and establishing equivalencies until they illuminate clearly the global connections of the early modern era.

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AHR Forum
Political Economy and Ecology on the
Eve of Industrialization:
Europe, China, and the Global Conjuncture

KENNETH POMERANZ

THIS ARTICLE COMBINES one familiar and one unfamiliar project. The first involves bringing our knowledge of Chinese economic history closer to parity with what we know about Europe, largely by making estimates for consumption, income, and availability of natural resources. The results suggest that many important economic variables had similar values in the more advanced parts of China and Europe circa 1750. Even more surprising, they suggest that, despite their very dense populations, the Yangzi and Pearl River deltas in 1750 were not facing appreciably worse ecological pressures than those faced by the most developed areas in Europe: thus these strains cannot, by themselves, explain much of the huge divergence between East and West in the nineteenth century.¹

Thus a second, more unusual, project: to use Chinese experiences to examine Europe. I take two eighteenth-century cases that are conventionally treated as already set on opposing paths—toward dramatic growth in Europe and stagnation in China—and find much that they shared, suggesting that their divergence was a discontinuous and partly exogenous development.

Thirty years ago, the European side of this divergence was described in terms of an “Industrial Revolution” with several agreed-upon features. First, it constituted a fundamental and fairly sudden break with “pre-industrial” times. Second, it was British in origin, with new best practices later diffusing to the continent. Third, its

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¹ The Yangzi Delta, consisting of the core prefectures of what G. William Skinner, “Cities and the Hierarchy of Local Systems,” in Skinner, ed., *The City in Late Imperial China* (Stanford, Calif., 1977), calls the Lower Yangzi macro-region, had some 36.5 million people in 1770 as its borders were defined by Yeh-chien Wang, “Secular Trends of Rice Prices in the Yangzi Delta, 1638–1935,” in Thomas G. Rawski and Lillian M. Li, eds., *Chinese History in Economic Perspective* (Berkeley, Calif., 1992), 35–68; I will sometimes use a slightly more restrictive definition, yielding an area with 31.5 million. Either way, it is clearly large enough to bear comparison with European nations of the time, in spite of not being an independent political unit. For more on the desirability of comparing China to Europe as a whole (rather than to individual countries), and parts of China to European states, see R. Bin Wong, *China Transformed: Historical Change and the Limits of European Experience* (Ithaca, N.Y., 1997).

essence was a string of spectacular breakthroughs in a few key industries (first cotton, then coal, then iron, steel, and land transport), rather than the steady but more modest gains in many other activities. Finally, Britain's foreign trade was central—especially for textiles—with some (though far from all) scholars emphasizing colonies and slavery.

But the literature since then has questioned all of this. Increasingly, European industrialization appears as just part of long, slow processes: market expansion, division of labor, many small innovations, and millions of people accumulating small profits. And since this gradual European story begins well before Europe had much extra-continental trade, and includes countries for which such trade never mattered as much as for Britain (much less Lancashire), it is much less global than the old British one.

In a recently published book, I argue that this picture is misleading—not because Europe's gradual market-driven growth did not matter but because it does not differentiate Europe from East Asia (or perhaps other places). Smithian dynamics worked just as well in China as in Western Europe, but they did not transform basic possibilities—eventually, highly developed areas faced serious resource constraints, in part because commercialization and handicraft industry also tended to accelerate population growth. Europe's escape required new technologies plus coal, New World resources, and various favorable global conjunctures—or, more properly, Britain's escape, since proto-industrialization in places such as Flanders and even Holland led to results more like the Yangzi Delta or Japan's Kantō plain than like England. (Not to mention Denmark, where very labor-intensive solutions to similar ecological pressures yielded agrarian prosperity, but with little growth of even handicraft industry until after 1850, and falling returns per labor hour.)² Industrialization did not follow naturally from any region's proto-industrialization; we can as easily see Europe as “China manqué” as vice versa. On the other hand, since I will argue that the most advanced parts of China in many ways resembled parts of the European mainland that initiated mechanized industrialization within a few decades of Britain—and are no longer seen, as they once were, as having been “blocked” from development by virtue of that relatively short lag—I would argue that our histories of Chinese core regions should also move away from too strong a focus on supposed blockages and developmental blind alleys.³

In a particularly powerful “gradualist” account, Jan de Vries has subsumed the Industrial Revolution in what he calls the “industrious revolution.” In the first phase of this process, which spanned roughly 1550–1850, households in northwestern Europe worked more hours and allocated more of their labor to production for the market, while saving time for that labor by purchasing some things that they once made for themselves. The industrious revolution, then, involves both increas-

² Thorkild Kjærgaard, *The Danish Revolution 1500–1800: An Ecohistorical Interpretation* (Cambridge, 1994), 151–54, 158, 160.

³ For a pioneering work that helped reverse the focus on “backwardness” and “failure” in France, see Patrick O'Brien and Caglar Keyder, *Economic Growth in Britain and France 1780–1914: Two Paths to the Twentieth Century* (London, 1978).

ing labor (a result of preferences shifting toward various goods over leisure) and Smithian specialization, with the expected gains in efficiency.⁴

But this describes the more commercialized parts of sixteenth to eighteenth-century China (and probably Japan, too) as well as it describes eighteenth-century Europe. Thus European industrialization must still need a separate explanation. I will argue below that the reasons why the industrious revolution played out so differently in European and East Asian cores have less to do with economic institutions, attitudes, or demographic processes in these core regions themselves than with the fortuitous location of coal, and with the very different, politically structured, relationships between these cores and their respective peripheries. (Of course, it also had something to do with the process of invention itself, but—to put things briefly—the important differences there seem to be external to the economy per se.)

Thus this essay has four parts: a discussion of consumption levels, an analysis of Chinese labor markets and household labor allocation, a discussion of possible ecological “limits to growth” at both ends of eighteenth-century Eurasia, and a discussion of why China’s industrious revolution appears to have stalled at roughly the same time that both population and per-capita production in Europe began to grow faster, while some ecological indices that had been declining stabilized.

JAN DE VRIES’ INDUSTRIOUS REVOLUTION helps resolve a paradox. The grain-buying power of Europeans’ per-hour or per-day wages fell sharply between about 1430 and 1550, and it did not return to 1350 levels until 1840 or later (depending on the country).⁵ Yet comparing death inventories over the same period (especially after 1650) shows clear increases in what ordinary people owned. These trends are compatible because people increased the time they spent working for the market; this let them buy both consumer durables and their increasingly expensive bread. This may have decreased people’s leisure time. It certainly decreased the time they spent making things for their own households: instead of making, say, their own candles, people specialized in weaving and bought their candles with cash.

The same thing was happening in China. The rice-buying power of day laborers’ wages probably fell from about 1100 on,⁶ but even ordinary people seem to have increased their consumption of “non-essentials,” especially between 1500 and 1750.

⁴ Jan de Vries, “The Industrial Revolution and the Industrious Revolution,” *Journal of Economic History* 54, no. 2 (1994): 249–70.

⁵ Fernand Braudel, *Civilization and Capitalism, 15th–18th Century*, Vol. 1: *The Structures of Everyday Life*, Siân Reynolds, trans. (New York, 1981), 134–35; Wilhelm Abel, *Agricultural Fluctuations in Europe from the Thirteenth to the Twentieth Centuries*, Olive Ordish, trans. (New York, 1980), 121, 136, 161, 199; Gregory Clark, “Yields per Acre in English Agriculture 1250–1860: Evidence from Labour Inputs,” *Economic History Review* 44, no. 3 (1991): 446. Recent unpublished research by Peter Lindert and Philip Hoffman and by Robert Allen that adjusts standard real wage indices for early modern Europe by factoring in housing costs suggests that the trends for most people may have been even worse than those suggested by the “grain wage” alone.

⁶ Kang Chao [Zhao Gang], “Zhongguo lishishang gongzi shuiping de bianqian,” *Zhonghua wenhua faxing yuekan* 16, no. 9 (September 1983): 57. There are some problems with the way Zhao makes his argument—most importantly that he sometimes reports only cash wages, ignoring what was often a large in-kind supplement—but the general trend is probably nonetheless correct.

Many of these are the same non-essentials as in Europe: tobacco, sugar, more and better clothes, eating utensils, and so on.

But first let us consider basic foodstuffs. Most estimates of caloric intake in eighteenth-century China compare well with Europe, whether we take averages for the whole population or figures for the hardest-working laborers.⁷ Comparable or even superior nutrition is also suggested by the rough parity between rural Chinese and English life expectancies around 1750, with both higher than most figures for continental European populations.⁸ Moreover, recent studies suggest that Chinese birth rates were equal to or lower than European ones throughout the 1550–1850 period,⁹ while the overall rate of population growth was first faster (1550–1750) and then similar (1750–1850);¹⁰ this also indicates that Chinese death rates were probably lower.

Poor Chinese reached these nutritional standards without spending any more of their incomes on basic foodstuffs than did poor Europeans. Fang Xing estimates that Yangzi Delta farm laborers (the poorest non-beggars in the region) spent 55 percent of their earnings on basic grain supplies in the 1600s and very slightly less in the early 1800s.¹¹ Henry Phelps Brown and Sheila Hopkins come up with 53 percent for the rural English poor in the 1790s.¹² Moreover, Fang's method of calculation almost certainly understates both household earnings (he omits women's earnings entirely, for instance) and misses many non-grain expenditures.¹³

Chinese could have simply buried their "extra" income under the house, but

⁷ Kenneth Pomeranz, *The Great Divergence: China, Europe, and the Making of the Modern World Economy* (Princeton, N.J., 2000), 38–39. See also Ming-te Pan, "Who Was Worse Off?" Preliminary draft of paper delivered at 1998 meeting of Chinese Historians in the United States (typescript in possession of the author), 10–11; Robert B. Marks, "Rice Prices, Food Supply, and Market Structure in Eighteenth-Century South China," *Late Imperial China* 12, no. 2 (1991): 77–78. See Gregory Clark, Michael Huberman, and Peter H. Lindert, "A British Food Puzzle, 1770–1850," *Economic History Review* 48, no. 1 (1995): 223–26; Ming-te Pan, "Rural Credit Market and the Peasant Economy (1600–1949)—The State, Elite, Peasant, and Usury" (PhD dissertation, University of California, Irvine, 1994), 327, for some of the key figures. For some French figures, which are considerably worse, see Maurice Aymard, "Toward the History of Nutrition: Some Methodological Remarks," in Robert Forster and Orest Ranum, eds., *Food and Drink in History* (Baltimore, 1979), 6–7.

⁸ Compare William Lavelly and R. Bin Wong, "Revising the Malthusian Narrative: The Comparative Study of Population Dynamics in Late Imperial China," *Journal of Asian Studies* 57 (August 1998): 714–48 (especially Table 2 and Figure 3); and James Lee and Cameron Campbell, *Fate and Fortune in Rural China: Social Organization and Population Behavior in Liaoning, 1774–1873* (Cambridge, 1997), 79; with E. A. Wrigley and R. S. Schofield, *The Population History of England, 1541–1871: A Reconstruction* (London, 1981), 230, 708–13 (and see Peter Razzell, "The Growth of Population in Eighteenth Century England: A Critical Reappraisal," *Journal of Economic History* 53 [December 1993]: 757–63, for a suggestion that these figures are too high; Razzell's suggested adjustment for infant mortality alone would bring a life expectancy at birth of 37.0 down to somewhere between 31.6 and 34.0). For continental examples, see John Knodel, *Demographic Behavior in the Past* (Princeton, N.J., 1988), 68–69; and Yves Blayo, "La mortalité en France de 1740 à 1829," *Population* (November–December 1975): 138–39 (showing a lower life expectancy in France).

⁹ Li Zhongqing, "Zhongguo lishi renkou zhidu: Qingdai xingwei ji qi yiyi," in Li Zhongqing and Guo Songyi, eds., *Qingdai huangzu renkou xingwei de shehui huanjing* (Beijing, 1994), 3.

¹⁰ Li Bozhong, "Kongzhi zengchang yi bao fuyu—Qingdai qian, zhongqi Jiangnan de renkou xingwei," *Xin shixue* 5, no. 3 (September 1994): 32–34; compare Colin McEvedy and Richard Jones, eds., *Atlas of World Population History* (New York, 1978), 28–29.

¹¹ Fang Xing, "Qingdai Jiangnan nongmin de xiaofei," *Zhongguo jingji shi yanjiu* 11, no. 3 (1996): 93, 95.

¹² Henry Phelps Brown and Sheila V. Hopkins, *A Perspective of Wages and Prices* (London, 1981), 14.

¹³ Pomeranz, *Great Divergence*, 137 and n. 110.

they did not.¹⁴ Numerous domestic commentators described (and usually decried) increases in popular consumption; lists of products in local histories and in fiction that was meant to be realistic describe a broad range of goods for sale even in rather remote towns; other texts describe the food, clothing, and home furnishings of families at various social levels.¹⁵ We also have accounts from various European visitors, most of whom (before 1800) compare levels of consumption favorably with those back home.¹⁶

Quantitative estimates confirm these impressions. They are necessarily inexact, but I have tried hard to make them conservative, and still came up with surprisingly high numbers (see Tables 1 and 2).¹⁷ In each case, Chinese per capita consumption seems at least comparable to Europe's at the same or a later date; this is no great surprise for tea and silk but is quite unexpected for sugar and total cloth. And despite numerous data problems, per-capita cloth output for the Yangzi Delta in 1750 appears close to that for England in 1800.

Similar numbers may have different meanings. But here, too, I see broad similarities over the sixteenth to eighteenth centuries—the urbanization of elites, decline of retinues as a main mark of status, published guides to consumption, a long series of ineffective sumptuary laws (which are not even updated in China after about 1550). Peter Burke, a leading historian of early modern European consumption, has concluded that the Chinese and Japanese sources available in translation suggest more East-West similarities than differences, at least at the elite level.¹⁸

¹⁴ Charles P. Kindleberger, "Spenders and Hoarders," in Kindleberger, ed., *Historical Economics: Art or Science?* (Berkeley, Calif., 1990), 35–85, does indeed suggest that Chinese were "hoarders" rather than "spenders," but gives little evidence for this.

¹⁵ Particularly striking accounts may be found in the novels *Jin ping mei* and *Xingshi yinyuan zhuan*—striking in part because they deal with a medium-sized city and a small town, respectively, in North China rather than with any of the country's great metropolises. For some reflections on consumption in China by a leading historian of early modern European consumption, see Peter Burke, "Res et Verba: Conspicuous Consumption in the Early Modern World," in John Brewer and Roy Porter, eds., *Consumption and the World of Goods* (London, 1993), 148–61. I deal with this at much greater length in Pomeranz, *Great Divergence*, 127–52.

¹⁶ For instance, George Staunton, *An Authentic Account of an Embassy from the King of Great Britain to the Emperor of China*, 3 vols. (Philadelphia, 1799), 2: 48; George Macartney (1793) in J. L. Cranmer-Byng, ed., *An Embassy to China: Being the Journal Kept by Lord Macartney during His Embassy to the Emperor Ch'ien-lung, 1793–1794* (London, 1962), 225; Louis Dermigny, *La Chine et L'Occident: Le commerce à Canton au XVIII^e siècle, 1719–1833*, 3 vols. (Paris, 1964), 3: 1253; Gaspar da Cruz in C. R. Boxer, ed., *South China in the Sixteenth Century, Being the Narratives of Galeote Pereira, Fr. Gaspar da Cruz, O.P. [and] Fr. Martin de Rada. O.E.S.A. (1550–1575)* (London, 1953), 106, see also 99.

¹⁷ By starting with the quantity of land reported on the tax rolls, we build in a big conservative bias, since under-reporting was chronic throughout China. I have used the highest estimates I could plausibly defend of the amount of land that was under basic grain crops, and, where estimating cash-crop production for an area was particularly tricky, I have simply omitted it from national totals, even though contemporaries may have remarked often that it produced the good in question. In the case of sugar, for instance, I have counted only output in Guangdong and Taiwan plus known imports, even though we know that mainland Fujian was also a major producer, and production scattered through the rest of China was estimated by a contemporary to be about one-ninth of the total of Guangdong, Taiwan, and that uncounted mainland Fujian output (cited in Christian Daniels, "Agro-Industries: Sugarcane Technology," Vol. 6, Part 3 of Joseph Needham, *Science and Civilization in China* [New York, 1996], section 42a: 97, 105). And within Guangdong itself, I have used a figure for the cash-cropping area more than 20 percent below that generated in Robert Marks's study of that province, and assigned only one-tenth of this cash-cropping area to sugarcane: a figure that Marks suggests is almost certainly too low. For further discussion, see Pomeranz, *Great Divergence*, 119–22.

¹⁸ Burke, "Res et Verba," 158.

TABLE 1
Tea and Sugar Consumption in China and Europe

Sugar in China circa 1750

3.8–5.0 lbs. per capita for country as a whole

(sugar use was more heavily concentrated in the Lower Yangzi, southeast coast, and Lingnan, where consumption may have been as high as 10 lbs. per capita)

Sugar in Europe per capita

| <i>Date</i> | <i>All Europe</i> | <i>Europe except Britain</i> | <i>Britain</i> |
|-------------|-------------------|------------------------------|----------------|
| 1680 | 1 lb. | .85 lbs. | 4 lbs. |
| 1750 | 2.2 lbs. | 1.90 lbs. | 10 lbs. |
| 1800 | 2.6 lbs. | 1.98 lbs. | 18 lbs. |

Tea in China circa 1840 (no earlier figures available)

.7 lbs. per capita

Tea in Europe per capita

| <i>Date</i> | <i>Britain</i> | <i>Non-Russian Europe (includes Britain)</i> |
|-------------|----------------|--|
| 1780 | 1.0 lb. | .12 lb. |
| 1840 | 1.4 lbs. | .25 lb. |

SOURCES: Production figures from Carla Rahn Philipps, "Trade in the Iberian Empires, 1450–1750," in James Tracy, ed., *The Rise of Merchant Empires* (Cambridge, 1990), 58–61 (Portuguese and Spanish colonies); and Neils Steensgaard, "Trade of England and the Dutch before 1750," in Tracy, 140 (for French, Dutch, and British colonies); Fernand Braudel, *The Structures of Everyday Life*, Siân Reynolds, trans. (New York, 1981), 251–52; Robert Gardella, *Harvesting Mountains* (Berkeley, Calif., 1994), 6, 38; Xu Dixin and Wu Chengming, *Zhongguo zibenzhu yi de meng ya* (Beijing, 1985), 99. European population figures from Colin McEvedy and Richard Jones, eds., *Atlas of World Population History* (New York, 1978), 28. British consumption figures from Sidney Mintz, *Sweetness and Power: The Place of Sugar in Modern History* (New York, 1985), 67, 73 (using 1700 figure for 1680). For Chinese calculations, see Kenneth Pomeranz, *The Great Divergence* (Princeton, N.J., 2000), chap. 3.

CHINA'S HIGH STANDARD OF LIVING could conceivably have been produced by institutions that were a barrier to further development: this is often implied by scholars who refer to "involution" or a "high-level equilibrium trap."¹⁹ But there is no convincing evidence that factor markets in either eighteenth-century China or Western Europe were clearly closer to neo-classical ideals than the other. Land was generally less encumbered in China and guild restrictions on artisanal activities far less important.²⁰ European capital markets were better places to raise really large sums of capital, but the relevance of this to productive activity prior to the railroad era was limited: Europe's biggest debtors borrowed mostly for war-making (and overseas colonization, on which more later). Chinese interest rates were higher, perhaps in large part because penalties for default were less severe; this combination of higher rates and lower risk may well have been preferred by the millions of households who made most of the investments for both agriculture and proto-industry. (Mechanized industry would have been profitable even at interest rates much higher than either Europe's or China's.)

¹⁹ See Philip C. Huang, *The Peasant Family and Rural Development in the Yangzi Delta, 1350–1988* (Stanford, Calif., 1990); Mark Elvin, *The Pattern of the Chinese Past* (Stanford, 1973). Elvin himself sees the barriers as more environmental than institutional, but others have changed the emphasis.

²⁰ I compare these at much greater length in Pomeranz, *Great Divergence*, 69–107.

TABLE 2
Selected Comparisons of Cloth Output and Consumption

Yangzi Delta circa 1750

(omitting salt-producing prefectures)
 (population approximately 31 million)

| | <i>Output per capita</i> | <i>Amount Consumed Locally</i> |
|--------------|--------------------------|--------------------------------|
| Cotton cloth | 11.2–14.5 lbs. | unknown |
| Silk cloth | 2.0 lbs. | unknown |

China circa 1750*Output per capita*

| | |
|--------------|--|
| Cotton cloth | 6.2–8.3 lbs. (probably nearer low end) |
| Ramie | unknown (still almost 4 lbs. per capita in 1914–18, despite a general long-term trend toward cotton) |

Great Britain circa 1800*Output per capita**Consumed within Britain*

| | | |
|----------------------------------|-----------|----------|
| Cotton, linen, and wool cloth | 12.9 lbs. | 8.7 lbs. |
|----------------------------------|-----------|----------|

France circa 1789*Output per capita*

| | |
|----------------------------------|----------|
| Cotton, linen, and wool cloth | 8.4 lbs. |
|----------------------------------|----------|

Germany circa 1830*Output per capita*

| | |
|----------------------------------|----------|
| Cotton, linen, and wool cloth | 5.0 lbs. |
|----------------------------------|----------|

FOR SOURCES AND DISCUSSION of data problems, see Pomeranz, *Great Divergence*, Appendix F, plus a revision in Kenneth Pomeranz, "Beyond the East-West Binary," forthcoming in *Journal of Asian Studies* 61, no. 2 (May 2002). On ramie, see Xu Dixon and Wu Chengming, *Chinese Capitalism, 1522–1840*, Anthony Curwen, trans. (New York, 2000), 124.

The best-known argument that China's rural economy grew along self-limiting lines fundamentally different from Europe's is Philip Huang's argument about "involution." (Zhao Gang and Jack Goldstone have made different, though related, arguments.)²¹ Essentially, Huang claims that because China was so densely populated, people engaged in self-exploitation, working ever-increasing hours for minimal returns, as they tried to meet fixed consumption targets from their shrinking farms. But since (among other things) paddy rice yields far more per acre than wheat, we will see that land hunger may have been no worse in eighteenth-century China than in most of Europe. Huang's more promising argument is that because Chinese women were strongly discouraged from working outside the home, there was no market for their labor; and since they had to be fed anyway, their families pushed them into more and more hours of very low return, home-based

²¹ Kang Chao [Zhao Gang], *Man and Land in Chinese History: An Economic Analysis* (Stanford, Calif., 1986); Jack Goldstone, "Gender, Work and Culture: Why the Industrial Revolution Came Early to England But Late to China," *Sociological Perspectives* 39 (1996): 1–21.

work for the market (mostly textile production) without buying goods that would have decreased their domestic burdens. Thus here, the intensification of labor was not accompanied by any meaningful reallocation of time in response to the market (or much specialization) and did not create a mass market for manufactures: consequently, it led to “involution,” not development.²²

Huang’s argument is controversial; there is no room to rehearse the discussion in the present essay.²³ But two points from my own work are worth adding. First, the consumption estimates above make it doubtful that Chinese in 1750 were no further above subsistence than before. Second, Huang’s estimates of the returns to spinning and weaving—the basis of his argument that women’s work earned a sub-subsistence wage—are based on data from the 1690s, when cotton cloth prices reached one of their two lowest points in the entire 1450–1850 period, while raw cotton prices were relatively high.²⁴ Combining Huang’s estimates of physical productivity with more typical mid-eighteenth-century prices yields very different results. Most recently, I have discovered that Huang also made some serious arithmetic and measurement errors that throw some of his earnings estimates off by as much as a factor of ten.²⁵

The earnings of spinners still come out quite low. But as Huang himself notes, most spinning was done by young girls, not adult women (at least in the Lower Yangzi); and even in the most pessimistic scenario, 210 days of spinning a year would feed a girl all year round.²⁶ More optimistic scenarios yield enough earnings for an adult woman to feed herself and perhaps even a couple of small children. If a woman spun and wove, the same 210 days of labor would yield about twelve *taels* of income per year; at mid-century rice prices, this would buy about three times the typical consumption of an adult female. For another comparison, I assumed that a male agricultural laborer could have done twelve months a year of work and that, in addition to the cash wages reported in the sources, he would have received *all* his meals for the year. (Actual employers provided some but not all food.) Even with these assumptions, I came up with a range for male farm-workers’ earnings that ranged from about 15 percent above the hypothetical spinner/weaver to 15 percent below.

In short, whatever other effects the culturally specific features of Chinese patriarchy may have had, it appears that, at least in this period, women’s earnings *more* closely approximated men’s than in Europe.²⁷ Thus there was every reason for Chinese families to consider the opportunity costs of both men’s and women’s time in making their purchases, and there are many indications that they did. So to a

²² Huang, *Peasant Family and Rural Development in the Yangzi Delta*, 91, 110.

²³ See, for instance, the summary in R. Bin Wong, “Chinese Economic History and Development: A Note on the Myers-Huang Exchange,” *Journal of Asian Studies* 51, no. 3 (1992): 600–11. I add a number of further points in Kenneth Pomeranz, “Beyond the East-West Binary,” *Journal of Asian Studies* 61, no. 2 (2002), which also includes a response to Huang’s objections to my work.

²⁴ Zhang Zhongmin, *Shanghai cong Kaifa dao Kaifang, 1369–1842* (Kunming, 1988), 207–08; compare Huang, *Peasant Family and Rural Development in the Yangzi Delta*, 84–86.

²⁵ See Pomeranz, *Great Divergence*, Appendix E; and Pomeranz, “Beyond the East-West Binary.”

²⁶ Xu Xinwu, ed., *Jiangnan tubu shi* (Shanghai, 1992), 469. Page 215 suggests that a woman and her underage helper (counted as half a laborer) did 265 adult equivalent days of textile labor per year, which would suggest about 180 days for the adult woman.

²⁷ For English data, see Sara Horrell and Jane Humphries, “Women’s Labour Force Participation and the Transition to the Male-Breadwinner Family, 1790–1865,” *Economic History Review* 48, no. 1 (1995): 102–03.

rising standard of living, we should add, at least provisionally, a calculating approach to using the family's resources. Thus the Chinese and European pictures look quite similar, both for production and consumption.

But these resemblances did not last. Between 1750 and 1900, both population and per-capita consumption soared in Europe. But in China, population growth slowed significantly by 1800, and per-capita non-grain consumption declined: early twentieth-century figures for cloth, sugar, and tea are well below even my most conservative estimates for 1750.²⁸ As we shall see later, this is not because the eighteenth-century estimates were too high.

ECOLOGICAL DIFFERENCES explain much of this divergence—but *not* because, as some people have suggested, the most developed parts of China were uniquely “over-populated.” Rather, Malthusian pressures seem to have been about equally relevant to core regions at both ends of Eurasia (as comparable life expectancies and living standards suggest). I will briefly review them in terms of Thomas Malthus's four necessities that compete for land: food, fuel, fiber, and building materials.

In neither place was a shortfall in food production imminent, although in Britain there was not much room left to expand agricultural production without either exhausting the soil or using techniques that were not yet available in 1800 (such as mined or synthesized fertilizer). Much of mainland Europe still had lots of slack capacity, thanks to institutions that encouraged too much fallowing, delayed the draining of swamps, and slowed the spread of mixed husbandry in pre-Napoleonic Western Europe (and even longer further east). From a Chinese perspective, this looks like a surprisingly slow spread of best practices due to peculiar institutional rigidities.²⁹ Britain had adopted these changes more readily, to the point where there was little more improvement to be expected from them on the eve of a greater than ever population boom: indeed, British agricultural yields changed very little between 1750 and 1850.³⁰ The only available methods for increasing per-acre yields still further in an ecologically sustainable way were, like those used in Denmark, highly labor intensive—so much so that England's profit-seeking, labor-hiring farmers would not have undertaken them—and these still created only limited gains.³¹

Even in dry-farming North China (generally a much more vulnerable ecosystem

²⁸ See, for instance, the estimate of roughly 2.2 pounds of sugar consumption per capita for the 1930s cited by Daniels, “Agro-Industries,” section 42a: 85. Chang Chung-li, *The Income of the Chinese Gentry* (Seattle, 1962), 303, cites a 1930s estimate for tea consumption of 1.3 pounds, which would be much higher than my estimate for 1840; but the 1840 estimate, because it counts only tea that entered long-distance trade and paid internal customs, is surely an underestimate.

²⁹ See Pomeranz, *Great Divergence*, 73–80.

³⁰ F. M. L. Thompson, “Rural Society and Agricultural Change in Nineteenth-Century Britain,” in George Grantham and Carol S. Leonard, eds., *Agrarian Organization in the Century of Industrialization: Europe, Russia, and North America* (Greenwich, Conn., 1989), 189, 193; Clark, “Yields per Acre in English Agriculture,” 456–59; Brinley Thomas, “Food Supply in the United Kingdom during the Industrial Revolution,” in Joel Mokyr, ed., *The Economics of the Industrial Revolution* (Totowa, N.J., 1985), 145–46.

³¹ On Denmark, see Kjærgaard, *Danish Revolution*, 37–38, 55–56, 123, 151–58; on the difference between capitalist and peasant strategies for dealing with ecological strain, see Mauro Ambrosoli, *The*

TABLE 3
Ecological Comparisons for Parts of China and Europe, circa 1800

| Soil Fertility and Nitrogen Flux Comparisons | |
|---|--|
| <i>Total wheat yields over 6 years</i> | |
| England | 2,092 kg/acre |
| North China | 1,836 kg/acre |
| <i>(Note: If one adds the three soybean crops on the North China plot, versus two clover crops for the British crop, the North China land is probably a better total food producer.)</i> | |
| <i>Nitrogen depletion by wheat crops</i> | |
| England | 44.77 kg/acre |
| North China | 42.49 kg/acre |
| <i>Nitrogen added to soil by manuring</i> | |
| England | 4,000–5,600 kg/cropped acre x .6%–4.9% nitrogen content (assuming most manure from cows) |
| North China | 5,600–8,900 kg/cropped acre x 2.0–7.5% nitrogen content (assuming mostly pigs) |
| <i>(Note: percentages of nitrogen content are for fresh manure, and decline sharply with time. Since North China farmers tended to add little bits of manure every few days, while English farmers more often did a massive application of fertilizer once or twice a year (using an animal-pulled cart to save labor), the Chinese fertilizer probably had an additional advantage not measured here.)</i> | |
| <i>Nitrogen-fixing crops</i> | |
| England | 2 crops of clover at an average of 60 kg nitrogen/acre |
| North China | 3 crops of soybeans at an average of 48 kg nitrogen/acre per crop |
| <i>(Note: very wide observed variation around mean for individual cases of both clover and soybeans—relatively little is known about what determines these variations.)</i> | |

SOURCE: Pomeranz, *Great Divergence*, Appendix B.

than South China), our limited data suggest, the nutrient balances for grain growing were more favorable than in England circa 1800.³² (They were probably less favorable for North China's cotton lands—of which more later.) And in China's rice-growing areas, known techniques could still raise yields without exhausting the soil.³³ (See Table 3.)

Both fuel and building needs drew heavily on forests. Here we might assume that China's cores would be far worse off than Europe's, given their denser population and the country's horrible deforestation in the late nineteenth and twentieth centuries, but this seems not to be true around 1750 or even 1800. The British Isles already had severe wood shortages before 1650, as did northern Italy; by 1800, Britain had perhaps 5 percent forest cover, and the rest of "insular and peninsular Europe" about 10–15 percent.³⁴ Even France, which was relatively well-forested by West European standards, was about 16 percent forest in 1789—compared to about 33 percent in 1550.³⁵ This meant that, even if no wood were ever wasted, France by 1789 would have needed about 90 percent of its annual

Wild and the Sown: Botany and Agriculture in Western Europe, 1350–1850, Mary McCann Salvatorelli, trans. (Cambridge, 1997).

³² Pomeranz, *Great Divergence*, Appendix B and chap. 5.

³³ Li Bozhong [Po-chung Li], *Agricultural Development in Jiangnan, 1620–1850* (New York, 1998), 119–27.

³⁴ Michael Williams, "Forests," in B. L. Turner II, et al., eds., *The Earth as Transformed by Human Action* (New York, 1990), 180–81.

³⁵ J. P. Cooper, "In Search of Agrarian Capitalism," in T. H. Aston and C. H. E. Philpin, eds., *The Brenner Debate: Agrarian Class Structure and Economic Development in Pre-industrial Europe* (New York, 1985), 139 n. 2.

forest growth just to meet people's minimum heating and cooking needs, leaving little for building, much less for expanding fuel-hungry iron forges (which often functioned only a few weeks a year for lack of fuel³⁶) or other industries.

For China, anecdotal evidence suggests that even in the extremely densely populated Lower Yangzi, the *ecological* effects of clearing the highlands did not become severe until about 1820. Wood was not plentiful in North China, but apparently few people were desperately short of fuel.³⁷ On the aggregate, the only figures I know of for 1700 yield a perfectly acceptable 37 percent forest cover for all of China proper, but disastrous deforestation was widespread by 1900. Interpellating for dates in between is tricky.

To try to fill the gap, I have done a rough reconstruction of land use for southwest Shandong circa 1800—an interesting area because it was quite densely populated but did not import much timber, and *was* horribly deforested as of the 1930s.³⁸ Despite making every effort I could to make the 1800 situation look bad, it came out much like that of France: 13 percent forest cover and a sustainable fuel supply per year about 20 percent above probable minimum needs.³⁹ This surely meant great hardship for many people, since distribution was uneven and wood was also needed for other uses, but this was also true in France.

But what of still more densely populated rice-growing China? Calculations are impossible for the Lower Yangzi, since we have no figures on this area's huge timber imports; but they can be done for Lingnan, China's second richest macro-region (focused on Canton). Lingnan has about 70 percent of France's land area; it had 17.5 million people in 1753 and 30 million in 1853. Yet even in 1853, Lingnan had considerably more forest than France in 1789; and although a far denser population relied on those trees, available wood per capita was double French levels in 1793 and still above France's 1789 levels in 1853. Thanks to a milder climate, fuel-saving cooking methods, and the burning of crop residues, the difference in wood available for non-fuel uses (assuming fuel needs were met first) was enormous: six times France's 1789 per-capita levels in 1793 and still more than double France's 1789 levels in 1853. So despite its denser population, various Chinese efficiencies seem again to suggest that China may have faced no more "Malthusian" stress than Europe as of 1800. (See Table 4.)

But these tables also show that, even with efficient fuel-gathering and use, population and proto-industrial growth were pressing hard on forest resources. Timber prices in both China and Europe were high and rising in the eighteenth

³⁶ Fernand Braudel, *Civilization and Capitalism, 15th–18th Centuries*, Vol. 2: *The Wheels of Commerce*, Siân Reynolds, trans. (New York, 1982), 367.

³⁷ Staunton, *Authentic Account*, 2: 141–42; Kenneth Pomeranz, *The Making of a Hinterland: State, Society, and Economy in Inland North China, 1853–1937* (Berkeley, Calif., 1993), 123–27; more details in Pomeranz, "The Making of a Hinterland: State, Society, and Economy in Inland North China, 1900–1937" (PhD dissertation, Yale University, 1988), Appendix F.

³⁸ Pomeranz, *Making of a Hinterland*, 123–37.

³⁹ Pomeranz, *Making of a Hinterland*, 124–25; Kenneth Pomeranz, "How Exhausted an Earth? Some Thoughts on Qing (1644–1911) Environmental History," *Chinese Environmental History Newsletter* 2, no. 2 (1995): 7–11; also see Pomeranz, *Great Divergence*, Appendix B.

TABLE 4
Wood/Fuel Supply Comparisons: Lingnan, France, and Southwest Shandong

| 4.1 | | | | | | |
|----------------------------------|--|---------------------------|---------------------|-------------------------------------|---------|---------|
| Date | Forested area (hectares) | | | Percent forested | | |
| | Guangdong | Guangxi | Lingnan | Guangdong | Guangxi | Lingnan |
| 1753 | 9,000,000 | 6,500,000 | 15,500,000 | 45 | 35 | 40 |
| 1773 | 8,200,000 | 6,020,000 | 14,220,000 | 41 | 32 | 37 |
| 1793 | 7,440,000 | 5,660,000 | 13,100,000 | 37 | 30 | 34 |
| 1813 | 6,560,000 | 5,240,000 | 11,800,000 | 33 | 28 | 30 |
| 1833 | 5,760,000 | 4,940,000 | 10,700,000 | 29 | 26 | 28 |
| 1853 | 4,880,000 | 4,700,000 | 9,580,000 | 24 | 25 | 24 |
| Comparison | | | | | | |
| France circa 1550 | | | | 33% forested | | |
| France circa 1789 | | | | 16% forested | | |
| | | | | (little further decline after that) | | |
| Southwest Shandong circa 1800 | | | | at least 13% forested | | |
| 4.2 | | | | | | |
| Date | Total Lingnan fuel supply per capita in tons of coal equivalent (tce) (if wood had no other uses) | | | | | |
| 1753 | 1.75 tce | | | | | |
| 1773 | 1.45 tce | | | | | |
| 1793 | 1.19 tce | | | | | |
| 1813 | .99 tce | | | | | |
| 1833 | .83 tce | | | | | |
| 1853 | .70 tce | | | | | |
| Comparison | | | | | | |
| France circa 1789 | | .64 tce | | | | |
| Southwest Shandong circa 1800 | | .62 tce | | | | |
| 4.3 | | | | | | |
| Date | Forest land (hectares) | Forest needed for fuel | Remaining forest | “Surplus” wood per capita (tons) | | |
| 1753 | 15,500,000 | 1,650,000 | 13,850,000 | 2.85 | | |
| 1773 | 14,220,000 | 1,675,000 | 12,545,000 | 2.25 | | |
| 1793 | 13,100,000 | 2,260,000 | 10,840,000 | 1.73 | | |
| 1813 | 11,800,000 | 2,469,000 | 9,331,000 | 1.32 | | |
| 1833 | 10,700,000 | 2,956,000 | 7,744,000 | 1.00 | | |
| 1853 | 9,580,000 | 3,339,000 | 6,241,000 | .74 | | |
| Comparison | | | | | | |
| France circa 1550 | | | | 3.6 tons | | |
| France circa 1789 | | | | .29tons | | |

FOR SOURCES AND METHODS OF CALCULATION, see Pomeranz, *Great Divergence*, Appendix C.

century,⁴⁰ and even if popular subsistence was not threatened yet, there were serious impediments to significant growth in per-capita energy use.

In Britain and Belgium, the wood crisis was greatly alleviated by a late eighteenth and nineteenth-century coal boom. However, mineral energy did not become central for most of Europe until quite late in the nineteenth century.

⁴⁰ For continental Europe, see, for example, Jack A. Goldstone, *Revolution and Rebellion in the Early Modern World* (Berkeley, Calif., 1991), 186; Ernest Labrousse, *Esquisse du mouvement des prix et des revenus en France au XVIII^{ème} siècle* (1933; rpt. edn., Paris, 1984), 343, 346–47, finding a larger price increase for fuel wood than for any other commodity in France between 1726 and 1789, with the rise continuing into the early nineteenth century. Britain is discussed later in this text. For China, see Li Bozhong, "Ming Qing shiqi Jiangnan de mucai wenti," *Zhongguo shehui jingji shi yanjiu* 1 (1994): 86–96.

Moreover, coal did not end the wood shortage, it just alleviated it—construction and the growing demand for paper kept European timber supplies very tight until North American imports eased the pressure. (Forested acreage roughly leveled off in Europe by 1850, but even that impressive holding action meant less wood per capita.)

The coal boom, as E. A. Wrigley has pointed out, represents a fundamental discontinuity. He calculates that the annual energy yield from British coal around 1820 (when output was five times that of 1750 and almost eight times that of 1700)⁴¹ was the equivalent of the sustainable yield from 15 million forest acres.⁴² A more standard conversion would make this 21 million “ghost acres”: more than all of Britain’s pasture and crop land combined.

This breakthrough required technical innovation *and* geographic good fortune. Huge coal seams with visible outcroppings lay relatively close to London: this provided both a rich and needy market and a pool of craftsmen who made crucial improvements in pumps, steam engines, and so on. By contrast, China’s best coal deposits lay in Shaanxi, several hundred landlocked miles from the Yangzi Delta: a bit like if Europe’s coal had mostly been under the Carpathian Mountains. The technical challenges also differed. British mines needed water pumped out constantly. For this, a coal-fired steam engine, which would later also solve the transport problem, was a great solution. Conversely, the availability of almost free coal at the pit-head made even the inefficient early steam engines worth deploying for this one use, and so worth tinkering with until they became efficient enough for use elsewhere. By contrast, China’s largest coal deposits were in mines where ventilation was a much bigger problem. Change these geophysical accidents and it becomes a lot harder to imagine such an early escape from the limits of an organic economy; it becomes a lot easier to see Western Europe as a potential Lower Yangzi, with growing ecological pressures eventually outstripping the gains from further division of labor.

Eighteenth-century Europe needed more fiber if far more people were going to have more clothes per capita and to ship cloth overseas in return for primary products. But raising more wool would simply take too much land away from more intensive uses. Flax is both very hard on the soil and very labor intensive. This made it a garden crop in much of Western Europe, something grown on a small scale in peri-urban areas with plenty of nightsoil and labor. Parliament repeatedly enacted heavy subsidies for flax during the seventeenth and eighteenth centuries, but British production rose very little, and continental production not much more (except in Russia, where the soil could be given a long rest after a couple of flax crops).⁴³ Matching the fiber supply that came from New World cotton by 1830 with domestic

⁴¹ Michael W. Flinn (with the assistance of David S. Stoker), *The History of the British Coal Industry*, Vol. 2: 1700–1830, *the Industrial Revolution* (Oxford, 1984), 26.

⁴² E. A. Wrigley, *Continuity, Chance, and Change: The Character of the Industrial Revolution in England* (New York, 1988), 54–55.

⁴³ Alex J. Warden, *The Linen Trade, Ancient and Modern* (1864; rpt. edn., London, 1967), 32–40; George Grantham, “Agrarian Organization in the Century of Industrialization: Europe, Russia, and North America,” in Grantham and Leonard, *Agrarian Organization in the Century of Industrialization*, 13–14; Jerome Blum, *Lord and Peasant in Russia: From the Ninth to the Nineteenth Century* (Princeton, N.J., 1961), 333–34. Note that Warden, writing in *The Linen Trade*, despaired of increasing British flax imports from the continent.

sources would have required an implausible thirty-fold increase in English flax production.

Cotton, the principal East Asian fiber source, is less labor intensive than flax, but it, too, is hard on the soil. The Lower Yangzi's huge imports of Manchurian soybeans went mostly to sustain cotton lands; so did most of Japan's vastly increased fishing after 1750. Europe, of course, eventually turned to cotton, too—not by growing it with imported fertilizer but by importing huge amounts of American cotton.

Thus fiber brings us to the more general issue of long-distance trade. As densely populated cores faced shortages of various land-intensive products, they sought them in less densely populated areas that could produce surpluses of timber, cattle, or grain but that produced few of the manufactures that cores had in abundance. Thus England and the Netherlands turned first to the Baltic (and the Mediterranean for cotton) and later to the New World; and the Lower Yangzi imported rice and timber from upstream, wheat and soybeans from Manchuria, and raw cotton from North China. The Yangzi Delta's trade for these primary products dwarfed anything elsewhere in the eighteenth-century world;⁴⁴ the Pearl River Delta was beginning to follow suit.

But this kind of trade tended to run up against limits: one more characteristic of East Asia, one of Europe. Where families in the peripheries were more or less free to allocate their own labor, an export boom and commercialization would often stimulate population growth, both from natural increase and immigration.⁴⁵ Moreover, as the best land filled up (or the most accessible forests were cleared), some labor would move into handicrafts: since most technology was not yet embodied in very expensive capital goods, and high transport costs on bulky items provided some protection for infant industries, this sort of import substitution was a much more “natural” process than today.⁴⁶ Together, these changes reduced raw materials surpluses for export and demand for imported manufactures.

This is precisely what happened in much of the Chinese interior in the late

⁴⁴ Food imports from the Middle Yangzi alone fed approximately 6 million people per year in the Lower Yangzi, and the soybeans that the Lower Yangzi imported could have fed at least another 3 to 4 million had most of them not been used as fertilizer. Even Shandong, a not particularly commercialized province with perhaps 23 million people in 1800 (Philip C. Huang, *The Peasant Economy and Social Change in North China* [Stanford, Calif., 1985], 322), imported enough food to feed 700,000 to 1 million people, and exported a like amount. By contrast, the Baltic grain trade fed about 600,000 people a year at its peak, and all of Europe's long-distance grain trade put together fed at most 2.5 million people at its pre-1800 peak. For numbers on these different flows, see Jan de Vries, *The Economy of Europe in an Age of Crisis, 1600–1750* (New York, 1976), 17, 56; Braudel, *Structures of Everyday Life*, 127; Adachi Keiji, “Daizuhaku ryūtsū to Shindai no shōgyō teki nōgyō,” *Tōyōshi Kenkyū* 37, no. 3 (1978): 35–63; Xu Dixin and Wu Chengming, *Zhongguo zibenzhuyi de mengya* (Beijing, 1985), 277; Xu Tan, “Ming Qing shiqi Shandong de liangshi liutong,” *Lishi dang'an* 57 (1995): 86; Marks, “Rice Prices, Food Supply, and Market Structure,” 76–79; Yeh-chien Wang, “Food Supply and Grain Prices in the Yangtze Delta in the Eighteenth Century,” in *The Second Conference on Modern Chinese Economic History*, 3 vols. (Taipei, 1989), 2: 423–30; Lu Hanchao, “Arrested Development: Cotton and Cotton Markets in Shanghai, 1350–1843,” *Modern China* 18, no. 4 (1992): 493; Li Bozhong, *Agricultural Development*, 113–14, 209 n. 35.

⁴⁵ Rising incomes tend to lower death rates and sometimes raise birth rates, too; increased demand for wage labor often makes earlier marriages possible than if couples have to wait to inherit property.

⁴⁶ I present the argument in a more formal model, adjusted to various possible assumptions about prices and other variables in Pomeranz, *Great Divergence*, 285–92. The model draws heavily on the work of Joel Mokyr, *Industrialization in the Low Countries, 1795–1850* (New Haven, Conn., 1976).

eighteenth and early nineteenth century. The Middle and Upper Yangzi grew very rapidly,⁴⁷ reducing their rice and timber surpluses; some of the extra hands available began making coarse cloth that replaced shipments from the Lower Yangzi.⁴⁸ In North China, population growth was so rapid that it probably required the reconversion of some cash-crop land to grain production;⁴⁹ and at any rate, much more of the region's huge raw cotton crop was spun and woven locally, rather than sent south.

To some extent, the Yangzi Delta compensated by finding new, more remote markets (in Manchuria, Southeast Asia, and to some extent in the West) and by specializing more in fancier fabrics for elites, moving up the value-added ladder as an established industrial area should. Nonetheless, it faced serious economic pressures that inhibited any further specialization in industry. I estimate (based on admittedly spotty data) that the rice-buying power of a hypothetical Yangzi Delta weaver/spinner fell by 22 to 42 percent between 1750 and 1800 (probably closer to 42 percent) and about 10 percent more by 1840.⁵⁰ Population growth in the delta was almost zero over this century, while China as a whole roughly doubled. Lingnan's regional core experienced a milder version of the same trends.⁵¹

These trends might have been less pronounced if people had migrated from the increasingly full peripheries to the Yangzi Delta—as they “should” have, given its higher standard of living. This might have allowed primary product exports from the peripheries to stay higher for longer; it also should have lowered Yangzi Delta wages, making its manufactured exports more competitive.

Here, however, Chinese institutions and values did matter. Cloth production was an overwhelmingly female activity, and women almost never migrated alone. They moved as part of male-headed households, and most rural men were farmers. Most industry was rural, and there were few places to live in the countryside for somebody who had neither kin to move in with nor access to land; this was not a landscape with great landlords looking for “cottagers.” Delta land was expensive, and even renting it often required a large deposit; thus poor couples from the interior had reasons to stay put unless they were completely landless.

On the other hand, Chinese institutions had been very successful (with government loans of seed, animals, etc.) in facilitating migrations of poor people toward areas with better land-to-labor ratios throughout the eighteenth and early nineteenth centuries: much better than in Europe, where institutional arrangements made land-rich Eastern Europe uninviting to any West European seeking a better life, while high migration costs limited poor people's migration to the pre-1800 New World to those willing to be indentured on terms that landowners found competitive with the chance to purchase slaves. As long as there was land to go to, facilitating those flows probably mattered far more to integrating labor

⁴⁷ G. William Skinner, “Regional Urbanization in Nineteenth-Century China,” in Skinner, *City in Late Imperial China*, 213; he then notes elsewhere the likelihood that this growth reduced the long-distance trade in rice between the eighteenth and twentieth centuries.

⁴⁸ Li Bozhong, *Agricultural Development*, 108; Yamamoto Susumu, “Shindai Shikawa no chi-iki keizai [Regional Development in Qing Dynasty Sichuan],” *Shigaku Zasshi* 100, no. 12 (December 1991): 7–8, 10–11; and for a general survey, Pomeranz, *Great Divergence*, 242–51.

⁴⁹ See Pomeranz, *Great Divergence*, Appendix F.

⁵⁰ See Pomeranz, *Great Divergence*, Appendix E.

⁵¹ Pomeranz, *Great Divergence*, chap. 6 and Appendix E.

markets than any flows toward the Yangzi Delta would have. Thus, on the whole, Chinese labor markets may still have been somewhat better integrated than Europe's.⁵² But as land frontiers disappeared, the difficulties of moving toward manufacturing or service jobs in highly developed regions became more significant. Meanwhile, the cultural ideal of the "man plows, woman weaves" household—which came to be realized far more often in the Qing dynasty,⁵³ although it was still often set aside where economic incentives to violate it were strong enough (as in the tea country)—meant that frontier families also produced cloth if possible. We thus see unfolding over time a phenomenon noted by Saito Osamu for Tokugawa Japan: the elaboration of a family division of labor that to some extent substituted for the deepening of geographic specialization.⁵⁴ As incomes rose in some of China's rice-exporting regions, and as Qing officials helped spread cotton growing and weaving in these regions, more families *could* keep their women employed inside, as they preferred; and, in doing so—rather than, for instance, doing more double-cropping of rice, as other Qing officials urged⁵⁵—they reinforced the ecological pressures on downstream areas.

This filling up of China's peripheries also helps explain why nineteenth-century observers did not record the decline in consumption that comparing my 1750 figures with early twentieth-century ones at first seems to suggest. Most areas did not undergo decline (the North and Northwest are likely exceptions),⁵⁶ but the changing weights of different areas dragged down national aggregates. The Yangzi Delta alone probably held 16 to 21 percent of China's population in 1750, but less than 9 percent by 1850 and under 7 percent by 1950. The three richest of G. William Skinner's eight Chinese macro-regions were over 40 percent of the population in 1750 and around 25 percent in 1843.⁵⁷ If, for instance, those three macro-regions

⁵² The same point could be made about other markets, too. The numerous ways in which both Europe and China differed from an ideal-typical market economy are important to bear in mind, lest we choose one Chinese barrier to growth and say "Aha! It was economic institutions after all!" It is also worth remembering that as an area like the Middle Yangzi—with perhaps 50 million people—began to develop its own proto-industry and internal trade, this was indeed a retreat from an integrated market on an even larger scale, including the Lower Yangzi, as I have emphasized here, but it also represented an elaboration of the division of labor and of Smithian dynamics on a scale that was still larger than that of any emerging national economy in Europe.

⁵³ Li Bozhong, "Cong 'fufu bing zuo' dao 'nan geng nu zhi,'" *Zhongguo jingji shi yanjiu* 11, no. 3 (1996): 99–107; Susan Mann, "Household Handicrafts and State Policy in Qing Times," in Jane Kate Leonard and John R. Watt, eds., *To Achieve Security and Wealth: The Qing Imperial State and the Economy, 1644–1911* (Ithaca, N.Y., 1992), 75–96.

⁵⁴ Saito Osamu, *Puroto-Kōgyō no jidai: Seitō to Nihon no hikakushi* (Tokyo, 1985).

⁵⁵ On Qing encouragement of cotton work in areas that did not have it, see Susan Mann, *Precious Records: Women in China's Long Eighteenth Century* (Stanford, Calif., 1997), 160–65, 176; on a failed campaign to increase double cropping in the Middle Yangzi, see Peter C. Perdue, *Exhausting the Earth: State and Peasant in Hunan, 1500–1850* (Cambridge, Mass., 1987), 131–35.

⁵⁶ Compare, for instance, the discussion of cotton in Fang Xing, "Lun qingdai qianqi mianfangzhi de shehui fengong," *Zhongguo jingji shi yanjiu* 2, no. 1 (1987): 79–94; and in Zhang Gang, "Qingdai Zhili shangpin jingji fenxi," *Hebei shiyuan xuebao* 3 (1985): 99; with Huang, *Peasant Economy and Social Change in North China*; Richard A. Kraus, "Cotton and Cotton Goods in China, 1918–1936: The Impact of Modernization on the Traditional Sector" (PhD dissertation, Harvard University, 1968); or Pomeranz, *Making of a Hinterland*. This is discussed at greater length in Pomeranz, *Great Divergence*, 140–42, 335–37.

⁵⁷ Calculated based on Skinner, "Regional Urbanization in Nineteenth-Century China," 213; G. William Skinner, "Sichuan's Population in the Nineteenth Century: Lessons from Disaggregated Data," *Late Imperial China* 8, no. 1 (1987): 67–76; and Liang Fangzhong, *Zhongguo Lidai hukou, tianli, tianfu tongji* (Shanghai, 1980), 395–413.

accounted for the overwhelming majority of the country's sugar consumption in the mid-eighteenth century—as they probably did—their falling share of China's population alone would account for almost all the decline between my figures and those in the John Buck surveys of the 1930s.⁵⁸ Living standards in many hinterlands may have continued to creep upward, but they were still far short of Yangzi Delta standards, and they came to dominate Chinese aggregates.

WE CAN NOW RECONSIDER how Britain escaped the Yangzi Delta's fate. One central factor was technological change—particularly steam and coal, which relaxed the land constraint in a more fundamental way than any other innovation before turn-of-the-century chemicals and electricity. But another part—at least as important as the small changes in numerous sectors that have been emphasized in recent decades—lay in its relations with its peripheries, which differed sharply from those we have just discussed.

The importance of these resources becomes even greater when we remember that, once begun, the mechanization of industry need not have been sustained, any more than previous bursts of growth had been; indeed, it could not have been sustained had what seemed to be pressing resource and environmental strains not been alleviated even while both population and per-capita consumption soared.

Western Europe's early modern trade with Eastern Europe was not squeezed by rising population and import substitution like that in the Chinese interior. East European serfdom and other institutions meant that agricultural improvement and population growth were slower than one would expect in a free-labor periphery: few people would immigrate from crowded but freer areas, and there was little of the wage labor that allowed people elsewhere to form families without inheriting land. Nor could peasants switch into handicraft activity on any great scale.

But these same institutions limited the response to export demand in the first place; they also limited the region's demand for imported manufactures, because so many people were very poor and/or outside the cash economy (even if their products were not). Thus the Baltic trade, for instance, leveled off after 1650, at a fraction of the size of China's long-distance staple trades.⁵⁹ This stagnation left slack export capacity waiting to be activated when changes in institutions, technology, and prices made the logic of selling grain to the West and buying its manufactures irresistible; but this mostly happened after 1860.⁶⁰

In the crucial hundred years before 1860, the New World did much more to relax northwest Europe's land constraint: both its natural bounty and its history facilitated this. Old World diseases removed millions of indigenes, and much of the

⁵⁸ The effect of the regional redistribution of population alone would lower an average consumption of 4.3 pounds to about 2.5, and John Lossing Buck, *Land Utilization in China* (1937; rpt. edn., New York, 1964), reported average consumption of centrifugal sugar of 2.2 pounds. Sugar processed in other ways, plus what was eaten raw in producing areas (where sucking on cane was common) could easily make up the remaining difference.

⁵⁹ Z. S. P. Pach, "The East-Central European Aspect of the Overseas Discoveries and Colonization," in Hans Pohl, ed., *The European Discovery of the World and Its Economic Effects on Pre-Industrial Society, 1500–1800* (Stuttgart, 1990), 186–88, 190.

⁶⁰ Thomas, "Food Supply in the United Kingdom during the Industrial Revolution," 141–50.

labor force was replaced by slaves—who were imported at a cost that consumed about one-quarter of export earnings in late eighteenth-century Brazil and the Caribbean.⁶¹ Moreover, these slaves often did not meet their own subsistence needs (unlike most coerced cash-crop workers in the Old World). Consequently, the circum-Caribbean slave region (from Brazil to the southern United States) became the first “modern”-looking periphery, with large bills for the import of capital goods (in this case, kidnapped ones) and for mass consumer goods (such as cheap cloth for slaves). Thus, unlike Old World peripheries, the New World kept expanding as a source of land-intensive exports, allowing Europe to become ever more specialized in manufacturing. (Manufactures were the bulk of the goods used to buy slaves in Africa, and they were also sold to North America, which earned much of the cash for its purchases with grain and timber for Caribbean plantations.⁶²)

In the long run, exports from free North America would be still larger, but that, too, mostly postdates 1860; and, as John McCusker and Russell Menard show, North American settlement was also tied for quite a while to the capacity to export.⁶³ For present purposes, consider how much New World commodities did to relax Britain’s land constraint, even as early as 1830. Replacing Britain’s 1801 consumption of Caribbean sugar with locally grown calories would have required 850,000 to 1.2 million acres of the best wheat land; by 1831—still before the great fall in sugar prices and quintupling of per-capita consumption that followed—the figure is 1.2 to 1.6 million. Enough wool to replace Britain’s American cotton imports in 1830 would have required over 23 million acres: more than either Britain’s total pasture and crop land⁶⁴ or E. A. Wrigley’s circa 1820 figure for the impact of coal.⁶⁵ Thus Britain got an extended window in which to solve certain resource constraints partly because markets *did not* work in its peripheries as well as they did in East Asia, thanks to bound labor, colonial monopolies, and such factors.

Land-saving New World imports kept soaring as industrialization proceeded, keeping pace with the central contribution of fossil fuels. Britain’s coal output

⁶¹ Calculations based on slave prices from Joseph C. Miller, “Slave Prices in the Portuguese Southern Atlantic, 1600–1830,” in Paul E. Lovejoy, ed., *Africans in Bondage: Studies in Slavery and the Slave Trade* (Madison, Wis., 1986), 70; numbers of slaves from Philip D. Curtin, *The Atlantic Slave Trade: A Census* (Madison, 1969), 216; and Armin K. Ludwig, *Brazil: A Handbook of Historical Statistics* (Boston, 1985), 314; Caribbean export figures from Noel Deerr, *The History of Sugar*, 2 vols. (London, 1949–50), 193–203 (British Caribbean), and 235–42 (French Caribbean); plus British import data from B. R. Mitchell, *British Historical Statistics* (New York, 1988), 462–64; Brazilian export figures for 1821–26 from Ludwig, *Brazil*, 107; and for 1796 and 1806 from Michel Morineau, *Incroyables gazettes et fabuleux métaux: Les retours des trésors américains d’après les gazettes hollandaises (XVI–XVIII^e siècles)* (New York, 1985), 177–78.

⁶² Herbert Klein, “Economic Aspects of the Eighteenth Century Slave Trade,” in James D. Tracy, ed., *The Rise of Merchant Empires: Long-Distance Trade in the Early Modern World, 1350–1750* (Cambridge, 1990), 291; James F. Shepherd and Gary M. Walton, *Shipping, Maritime Trade, and the Economic Development of Colonial North America* (New York, 1972), 43–44; David Richardson, “The Slave Trade, Sugar, and British Economic Growth, 1748–1776,” *Journal of Interdisciplinary History* 17, no. 4 (1987): 765–66.

⁶³ John J. McCusker and Russell R. Menard, *The Economy of British America, 1607–1789* (Chapel Hill, N.C., 1985), 18, 23, 28–30.

⁶⁴ About 18 million acres when first systematically counted in the mid-nineteenth century, though probably higher earlier; see Mitchell, *British Historical Statistics*, 186.

⁶⁵ See Pomeranz, *Great Divergence*, Appendix D, for derivation of all figures on “ghost acreage.”

would increase fourteen times from 1815 to 1900,⁶⁶ its sugar imports roughly eleven-fold,⁶⁷ and its cotton imports a stunning twenty times.⁶⁸ It also began to use huge amounts of American grain, beef, lumber, and other primary products; and the New World also became a vast outlet for surplus population from various parts of Europe. As these migrants brought with them European tastes, as technological progress created mechanical capital goods (rather than the enslaved human ones of an earlier era) in high demand across the Atlantic, and as independent New World governments emerged with their own reasons for paying the costs of frontier expansion, the various peculiar institutions that had helped create a flow of land-intensive New World exports were no longer important, but they had been crucial while the colonies and transatlantic trade were taking shape.

MANY READERS MAY RAISE ONE OF THREE related objections. All of them, in one form or another, hinge on the notion that no one resource is “vital”: as scarcity raises prices, people will find substitutes. Thus I may seem to be placing too much emphasis on coal, or forgetting that, however useful land-intensive New World products may have been, the majority of such resources still came from within Europe. Lastly, I may appear to be issuing a “Club of Rome” report for 1790: suggesting that, without the New World and coal, Europe was headed for a Malthusian crisis, when it probably would have adjusted through some combination of lower fertility, lower consumption, and the use of land and energy-saving techniques. I agree that this would have been a far more likely outcome than actual catastrophe, although there were signs of serious soil exhaustion and other problems in various regions.⁶⁹ I would, however, argue that the ecological adjustments possible without New World resources or such modern inputs as chemical fertilizer (itself based on fossil fuels) were sufficiently labor intensive that their widespread adoption would have made nineteenth-century European economic history very different—more like that of the richer parts of East Asia, or some unusual European cases like Denmark, than like England. Let us now consider these objections.

Coal was central to earlier views of the industrial revolution. Only cotton, iron, steel, and railways were equally emphasized, and three of these four other main sectors depended on coal. Much recent literature has deemphasized coal. People have noted, for instance, that water powered more early factories than coal, and that most of England’s coal was used for home heating and cooking. Even the calculations Wrigley uses to reaffirm the centrality of coal⁷⁰ cannot tell us what would have happened without the coal boom: presumably, it would have been some

⁶⁶ Mitchell, *British Historical Statistics*, 247.

⁶⁷ Calculated based on Mitchell, *British Historical Statistics*, 709–11.

⁶⁸ Mitchell, *British Historical Statistics*, 334–35. Imports peaked in 1912 at twenty-eight times 1815 levels.

⁶⁹ See Kjærgaard, *Danish Revolution* (Denmark); Ambrosoli, *Wild and the Sown* (parts of England); Piers M. Blaikie and Harold Brookfield, *Land Degradation and Society* (London, 1987) (parts of France and Germany). I take up the issue at more length in Pomeranz, *Great Divergence*, 215–25.

⁷⁰ Wrigley, *Continuity, Chance, and Change*, 54–55; see also Pomeranz, *Great Divergence*, 57–62.

combination of people being colder, buying more clothes, and producing less iron, rather than a complete blockage in any one sector.

But coal needs to be central to the story, for Wrigley's reasons and others. Water power may have kept expanding for a while, but it had inevitable geographic limits. Nor could it substitute for coal combustion in all sorts of chemical and physical processes (from brewing to metallurgy to dye-making), nor in fueling the railways and steamships that so greatly deepened the division of labor. In iron and steel, too, it is hard to see any adequate alternative to fossil fuels. Even under ideal conditions, all the woodland in England and Wales could have supported a maximum annual output of 87,500 to 175,000 tons of pig iron; but actual British output reached 400,000 tons by 1820.⁷¹ Other sectors would also have grown more slowly without such cheap and adaptable energy. Even the steam engine itself was at first sufficiently bulky, fuel-hungry, and dangerous that it was only worth deploying to pump water from coal mines, where fuel was virtually free ("small coals" that were not worth transporting beyond the pit-head)⁷²: without that use, and had the coal thus mined not then made fuel cheap more generally, further tinkering to improve the steam engine might not have seemed worth it. Coal does not explain the innovations it was used in, but without it no innovations could have made so much difference.

Similarly, one might object that arguments about New World resources have a weakness which parallels that of older arguments about overseas extraction and European capital accumulation: how can we call something decisive if other factors (for example, capital accumulation within Europe or domestic supplies of food) were larger? The question is important, and not only for this case.

If we are doing growth accounting for a single case, smaller factors are minor factors. But even here, defining categories creates questions. "New World farm goods imported to Britain, 1830" will look small next to "domestic farm production," but "fiber imports from the United States" would look quite large next to "all other fibers." And how specific we make our categories depends on complex judgments (and implicit counterfactuals) about the substitutability of different products, the importance of particular sectors for the larger economy, and so on. (Thus it seems much more likely that New World *resources* were crucial than that the New World profits stressed in some older literature was crucial⁷³; there were other profitable investment possibilities, but it is less clear that there were other

⁷¹ John R. Harris, *The British Iron Industry 1700–1850* (Houndmills, Basingstoke, Hampshire, 1988), 25, 56. G. Hammersley, "The Charcoal Iron Industry and Its Fuel 1540–1750," *Economic History Review*, 2d ser., 26, no. 2 (1973): 602–07, estimates the forest needs of iron production; calculations extending this are my own; see also M. W. Flinn, "Technical Change as an Escape from Resource Scarcity: England in the Seventeenth and Eighteenth Centuries," in Antoni Marczak and William N. Parker, eds., *Natural Resources in European History* (Washington, D.C., 1978), 139–64. On Britain's forest cover, see Michael Williams, "Forests," in Turner, *Earth as Transformed by Human Action*, 180–81. Flinn also points out that without coal, charcoal shortages could have hobbled the growth of English iron production *after* 1750 (p. 145); his emphasis is on showing that the earlier rate of output was sustainable, and that there was no worsening charcoal crisis that *caused* the development of coal-based iron-making. See also Pomeranz, *Great Divergence*, 60.

⁷² For more details on the relationship of coal, steam, and land transport, see Pomeranz, *Great Divergence*, 61–62, 65.

⁷³ Most famously, Eric Williams, *Capitalism and Slavery* (New York, 1944); see also Immanuel Wallerstein, *Capitalist Agriculture and the Origins of the European World-Economy in the Sixteenth Century* (New York, 1974).

ways to get huge amounts of land-intensive goods.) Unless we assume that there *must* be affordable substitutes for anything, such judgments cannot be avoided, and there will be cases in which small increments make large differences.

How important coal and the New World will seem depends partly on how convinced readers are of the similarities I have suggested in other areas. First, the calculations above show that these phenomena were not small relative to some reasonable standards (such as Britain's domestic land base); secondly, they appear at the right time to explain a crucial divergence (once we see that this divergence dates to the hundred years surrounding 1800); thirdly, they relieved a constraint—the finite amount of land—which was otherwise very difficult to relieve within the knowledge base and institutions of the time; and finally, the examples of core regions in China and Japan and certain parts of Europe itself (such as Denmark) provide plausible examples of how societies lacking these advantages might have looked. We need not imagine that, without this relief, Europe would have suffered a Malthusian catastrophe, or that, with a slightly longer ecological window, China would have industrialized on its own. (No place need have done so, which is one reason why asking, “Why wasn't England the Yangzi Delta?” may be a useful corrective to ideas derived from the opposite question.) A European ecological crisis *could* have happened, but we can also imagine some more likely outcomes, which could have preserved eighteenth-century living standards but would have been unlikely to lead to thoroughgoing industrialization, and might even have impeded it.⁷⁴

Without both fossil fuels and access to the New World, which together removed the need to manage land intensively, Europe, too, could have wound up on an “East Asian,” labor-intensive path. Indeed, there are many signs of such tendencies in eighteenth-century Europe: in the decline of meat eating from roughly 1400 to 1800, in certain aspects of English agriculture and proto-industry, and in almost everything about Denmark.⁷⁵ The East-West difference in labor intensity was not essential but contingent; take away the “resource shocks” of coal and the New World and it is not hard to imagine continued European convergence toward a much more labor-intensive world, in which many more people worked on the land, increasing yields while preserving fertility through more marling, more careful manuring, and more gathering of crop residues. Progress along such a path might well have maintained or even slightly improved living standards, but it would not have brought Europe any closer to our energy-intensive, capital-intensive world. Indeed, to the extent to which additional laborers on the land really were productive—so that removing them from farm work would push up agricultural prices—and to the extent that such labor-intensive “solutions” to land constraints gradually decrease the rewards for solving the problem in a different way, they

⁷⁴ For examples of how successful labor-intensive adjustments might make industrialization more difficult later, see Pomeranz, *Great Divergence*, 285–97.

⁷⁵ Braudel, *Structures of Everyday Life*, 196; and Catharina Lis and Hugo Soly, *Poverty and Capitalism in Pre-industrial Europe* (Hassocks, Sussex, 1979), 13–14, on declining meat consumption; Ambrosoli, *Wild and the Sown*, and David Levine, *Family Formation in an Age of Nascent Capitalism* (New York, 1977), on English agriculture and proto-industry; Kjærgaard, *Danish Revolution*, on Denmark.

could have made breakthroughs like the Industrial Revolution and the nineteenth-century version of the Agricultural Revolution⁷⁶ progressively less likely with time.

THE PROCESSES THAT HAVE BEST BEEN CAPTURED in the recent literature on early modern growth and “how the West grew rich” are important, but most of them are also the parts that Europe *shared* with some other parts of the early modern world. Those shared processes alone could have led to a Lower Yangzi result (or a Danish, Dutch, or Flemish result) instead of an English one: not because of any institutional “failures” but due to basic ecological realities and to limits on the ability of labor and capital to substitute for land in the era before fossil fuels, synthetic fertilizer, and the like. To explain East-West *differences*, we need to look at how those constraints were relaxed in Europe. Part of the story—which I have largely neglected here—is technological innovation; since we cannot take that for granted, we cannot argue that, with similar resource bonanzas, China would have had its own industrial revolution. But neither was inventiveness alone sufficient to relax the land constraint and create self-sustaining growth between 1750 and 1850; and without the land saving that coal and the New World provided (without being very labor intensive), one can imagine the focus of inventive efforts themselves being very different. Thus understanding the “European miracle” (once we place it back in the nineteenth century) requires that we look again at some topics from earlier scholarly generations—coal, empire, English exceptionalism, and the *discontinuity* of the industrial revolution—as they appear in a Chinese mirror.

⁷⁶ See F. M. L. Thompson, “The Second Agricultural Revolution, 1815–1880,” *Economic History Review*, 2d ser., 21, no. 1 (1968).

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The Search for European Differences and Domination in
the Early Modern World: A View from Asia

R. BIN WONG

ECONOMIC HISTORIANS WRITING ABOUT the sixteenth to eighteenth centuries have largely examined two clusters of topics, the first regarding the processes of change based on institutions and practices found in various European settings and the second centered on the construction of relations linking European actors with people in other parts of the world. Complementary themes emerge from these research activities. The former frequently affirms qualities that made early modern Europe different economically from other places. The latter usually rehearses components of a European system of world economic domination. These two typical foci of early modern economic history—what made Europe different and how Europe came to dominate the globe economically—suggest two analytical challenges: first, to compare more closely aspects of European economic practices with those found elsewhere and, second, to observe more carefully the economic connections that are forged during the early modern era.

In this article, I will first compare patterns of economic expansion in parts of Asia and Europe, followed by a contrast of Chinese and West European political economies. We will see some broadly parallel dynamics of commercial expansion embedded within distinctive political economies; the significance of the latter will be explored by posing a counterfactual in which key features of Chinese political economy are imagined to be different. I will then examine the kinds of trade connections that linked Asia and Europe in the early modern era and contrast them with those forged since the nineteenth century. Important differences will be suggested that qualify, if not undermine, any simple picture of a European world economy gradually growing to embrace the globe. The similarities among dynamics of early modern economic expansion at both ends of Eurasia together with differences between the types of logic typical of nineteenth-century international trade flows and those characterizing the earlier period promote a more skeptical attitude toward claims made about how different early modern European economic potentials were from those elsewhere and assertions of European economic dominance growing over much of the world at the same time. These comparisons and connections among early modern economies suggest that at least some, if not most, of the supposed European differences from and domination over others in

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economic matters may simply be false; they are at best weakly supported empirically.

The concluding section of this article underscores our need to specify more sharply the challenges of explaining changes between the world of pre-nineteenth-century economic activities and the industrial era that followed. Any number of political, cultural, and social differences can be noted between parts of Europe and parts of Asia between the sixteenth and eighteenth centuries. But demonstrating their causal significance to an economic divergence that only clearly takes place toward the late eighteenth and early nineteenth centuries has, to date, proven difficult. I certainly do not propose in the brief compass of this article to offer an explanation of the economic changes spanning the nineteenth-century divide that built on political, cultural, and social differences across Eurasia. My purpose here is more modest, though hopefully useful to constructing answers to those larger questions. If we can agree on important similarities in the early modern era—dynamics of economic expansion in many Asian and European settings and also accept important differences between international trade before and after the nineteenth century, we should have an incentive to move off those well-worn paths of exploration that have led so many scholars to look largely at European practices in order to identify their economic superiority and turn instead to territories less well traveled in order to better identify what did, after all, set parts of Europe and subsequently North America off from most of the rest of the world in terms of economic potential for at least a century.

THE WORLD OF PRODUCTION before 1800 was overwhelmingly agrarian. The vast bulk of the material wealth came from the land. Most of the rest resulted from the labors of people who lived in rural settings, even if they did not spend much time working the soil or tending animals. Across the New and Old Worlds, there were variations, to be sure, in the mix of crops, the relative importance of animal husbandry, and the institutions of agrarian life. Differences in levels of factor productivity reflected specific constellations of capital, labor, land, and technology. Some areas supported higher population densities and greater material well-being than others. But contrary to many assumptions made by scholars limited to European data, standards of living do not appear so very different across Asia and Europe, as Kenneth Pomeranz suggests in his part of the *Forum*. While there are some differences, they are not of a kind that allows easy inference toward some later trajectory of economic change.

In a world in which the range of production possibilities before 1800 was broadly the same everywhere, economies grew in two related ways. They grew through extension, most commonly by increasing the amount of land under cultivation or, in some cases, expanding the scale of rural manufactures. Alternatively, and more rarely, economies grew because the rates of factor productivity rose. The basic mechanism for achieving such gains was through the expansion of market exchange. As market areas grew and individuals began to specialize in those lines of production from which they could reap the greatest gains and to exchange their

goods for other items they wanted or needed, economies grew *and* individuals could improve their material well-being. This dynamic, made famous by Adam Smith, powered much of the economic growth taking place across the early modern world.¹

Because the European cases are better known, let's consider more fully some Asian examples. The early modern expansion of Japanese markets has often been taken to be the first step in a set of parallel changes with those in Europe that prepare us to understand how and why Japan burst forth as the industrial power it has in the twentieth century. Between 1500 and 1700, Japan became one of the most urbanized societies in the world. Driving urban development was the expansion of commerce and the imposition of political authority across larger areas. The premier commercial city of the country was Osaka, where wholesalers in sake, soy sauce, seed oils, and tea among many other commodities joined a shipbuilding industry with more than 2,000 carpenters and a copper-refining industry that processed some 3,000 tons annually, most of it for export overseas.² By the late eighteenth century, regional centers of production emerged for silk, lamp oil, and soy sauce to reduce the singular importance of older production centers such as Kyoto for silk and Kobe for lamp oil.³ The growth of commercial production and distribution created shifting regional specializations as commercial production gradually penetrated more deeply into rural areas throughout much of the country in the eighteenth century.⁴ However striking these commercial developments within Japan, they were hardly unique within Asia.

In Southeast Asia, the sixteenth and seventeenth centuries were an era of expanding maritime trade and urbanization. Port cities were connected by a maritime trading network in which merchants developed new credit schemes to facilitate increasing volumes of trade; silver became the principal source of money. Members of various merchant groups became important politically as they consolidated their wealth.⁵ In part stimulated by the growing maritime trade, land-based commerce at a growing number of smaller market towns brought peasants into contact with a larger world of goods. By the eighteenth century, local crafts were joined by goods carried in by maritime trade.⁶ Similar developments also took place in South Asia, where the findings of specialists about markets, merchants, and credit have been taken into account in the 1980s by Fernand Braudel in *The Wheels of Commerce*.⁷

In China, an initial commercial revolution began during the tenth century as

¹ I discuss Smithian dynamics of growth in both China and Europe at greater length in R. Bin Wong, *China Transformed: Historical Change and the Limits of European Experience* (Ithaca, N.Y., 1997), 16–21, 27–32, 38–42, 50–59.

² Nakai Nobuhiko and James L. McClain, "Commercial Change and Urban Growth in Early Modern Japan," in John Whitney Hall, ed., *The Cambridge History of Japan*, Vol. 4: *Early Modern Japan* (New York, 1991), 560.

³ Nakai and McClain, "Commercial Change and Urban Growth," 580–81.

⁴ Shinbo Hiroshi and Hasegawa Akira, "Shōhin seisan ryūtsu no dainamikusu," in Hayami Akira and Miyamoto Matao, eds., *Keizai shakai no seiritsu* (Tokyo, 1988), 217–70.

⁵ Anthony Reid, *Southeast Asia in the Age of Commerce, 1450–1680*, Vol. 2: *Expansion and Crisis* (New Haven, Conn., 1993), 62–131.

⁶ David Joel Steinberg, ed., *In Search of Southeast Asia: A Modern History*, rev. edn. (Honolulu, 1987), 49–59.

⁷ Fernand Braudel, *Civilization and Capitalism: 15th–18th Century*, Vol. 2: *The Wheels of Commerce*, Siân Reynolds, trans. (New York, 1982), 115, 122–25, 146–47, 153, 163, 380.

improvements in riverine transport combined with improvements in agriculture to support the expansion of merchant organizations and long-distance trade.⁸ The organization of markets, transportation networks, urbanization, and merchant practices have all been reconstructed from the primary sources available for the period.⁹ This initial burst of economic changes was followed by another major expansion of commerce in the sixteenth century. From the late sixteenth century, specialized market towns appear in Jiangnan, the east-central part of China near present-day Shanghai. Markets for cotton textiles, silks, and rice were central nodes of networks carrying these goods over much of the empire.¹⁰ In addition, there were more modest specialized markets for other goods, including salt, fish, bamboo and wood, pottery, metal goods, embroidery, tobacco, vegetable oils, writing brushes, and carts.¹¹ The marketing system of the Jiangnan region became the most complex within the empire, and is also the most thoroughly studied, because of the rich sources on the area. But there are also studies of markets across virtually the entire empire that confirm widespread commercialization.¹²

We also know that factor markets in China were developing in this period as well, although the evidence is less abundant and not as exhaustively studied as the information on product markets. Labor was hired both on a long-term basis and on a short-term basis for the busiest periods of the year.¹³ Much Chinese research effort has been expended determining how “free” or “feudal” hired labor may have been.¹⁴ From the point of view of market development, what is significant is that there was a supply of labor that could be hired to exploit a given land holding more efficiently. A family farm could adjust its labor supply to match more closely the labor requirements of its land by either hiring in labor or having sons hire out. Alternatively, peasants could buy or sell land, one effect of which was to alter the ratio of land to labor within the family.¹⁵ Some transactions were straightforward purchases and sales, but many others were more conditional sales wherein the party

⁸ Mark Elvin, *The Pattern of the Chinese Past* (Stanford, Calif., 1973), 113–99.

⁹ Shiba Yoshinobu, *Sōdai shōgyōshi kenkyū*, abridged trans. by Mark Elvin as *Commerce and Society in Sung China* (Tokyo and Ann Arbor, Mich., 1968, 1970).

¹⁰ Liu Shih-chi, *Ming Qing shidai Jiangnan shizhen yanjiu* (Beijing, 1987).

¹¹ Fan Shuzhi, *Ming Qing Jiangnan shizhen tanwei* (Shanghai, 1990), 248–61.

¹² Studies include Yamane Yukio, “Shindai Santō no shishū to shinshi sō,” *Tōyō gakuho* 6, nos. 1–4 (1985): 539–60 on Shandong markets; Liu Shih-chi, “Ming Qing shidai Jiangxi xushi he shizhen de fazhan,” in Mindai shi kenkyū kai and Mindai shi ronso henshū i-in kai, eds., *Mindai shi ronso* (Tokyo, 1990), 795–820 on Jiangxi; Saito Fuminori, “Min Shin jidai Fukuken no kyōshi ni tsuite,” in Mindaishi kenkyū kai and Mindai shi ronso henshū i-in kai, eds., *Mindai shi ronso*, 821–40 on Fujian markets; Morita Akira, “Shindai Kōkō chiho ni okeru teikishi ni tsuite,” *Kyūshū sangyō daigaku shokai ronshū* 5, no. 1 (1964): 49–73.

¹³ Liu Yongcheng, *Qingdai qianqi nongye zhibenzhuji mengya chutan* (Fuzhou, 1982).

¹⁴ Li Wenzhi, Wei Jinyu, and Jing Junjian, *Ming Qing shiqi de nongye zibenzhuji mengya wenti* (Beijing, 1983).

¹⁵ The presence of contracts for the purchase and sale of land make clear that there were land markets. Several documentary collections with land contracts have been published in China and Japan, including Tōyō Bunko Mindai shi kenkyū shitsu, ed., *Zhongguo tudi qiyue wenshuji (Jin–Qing)* (Tokyo, 1975); Sichuan daxue lishixi and Sichuan sheng dangan guan, eds., *Qingdai Qian Jia Dao Baxian dangan xuanbian* (Chengdu, 1989, 1996); Hamashita Takeshi, Kubo Toru, Ueda Makoto, Kishimoto Mio, Usui Sachiko, and Terada Hiroaki, eds., *Chūgoku tochi bunsho mokuroku, kaisetsu* (jo) (Tokyo, 1983); Hamashita Takeshi, Kubo Toru, Ueda Makoto, Kishimoto Mio, Usui Sachiko, Terada Hiroaki, Hayashi Masako, Takamizawa Osamu, Ishibashi Takao, and Yang Guozhen, eds., *Chūgoku tochi bunsho mokuroku, kaisetsu* (shita) (Tokyo, 1986). Research to date has largely analyzed the institutional contexts of contract making and considered how the contracts can inform a Marxist perspective on

selling the parcel of land retained the right to buy back the land at a stipulated price within some fixed number of years. From the vantage point of credit, these transactions can be considered as loans with the land as collateral, but since many of these conditional sales of land do not appear to have been redeemed, they end up being actual sales, usually recognized as such when the buyer paid an additional sum of money to complete the sale.¹⁶ These sales were complemented by different rental transactions. Together, the markets for land and for labor made it feasible for a family farm to grow and contract according to the life cycle of family. In practice, most families preferred to buy land when they could and, if they lacked sufficient labor power, to hire in some help rather than sell their holdings to a level they could manage on their own.¹⁷

Credit markets allowed merchants to borrow funds. Financial institutions of various types we collectively call “native banks,” in Chinese, *zhangju*, *piaohao*, and *qianzhuang*, often took deposits and made loans. The first *zhangju* appears to have been established in Zhangjiakou in 1736 by a Shanxi merchant who committed 40,000 *taels* of capital; it acted as a bank for deposits and loans to facilitate the Chinese-Russian trade.¹⁸ There is some debate about whether to date the beginnings of the *piaohao* to some point in the eighteenth century or the opening decades of the nineteenth century.¹⁹ At a minimum, the record shows that there was a considerable expansion of *piaohao* activity beginning in the early nineteenth century. They dealt in interregional remittances to facilitate the transactions of Shanxi merchants, who were located in the northern half of the empire as well as in the Jiangnan region. For example, when a buyer did not have the ready cash to pay for a considerable amount of jade in Suzhou, he issued a *huipiao*, or remittance draft, to be collected in Shanxi.²⁰ The 1985 discovery of twenty-three seventeenth-century remittance drafts covering the years 1683, 1684, and 1686 for a Huizhou merchant based in Beijing had a total value of 13,980 *taels*; these documents demonstrate both that the system of remittances was used by the other major

social relations; see Yang Guozhen, *Ming Qing tudi qiyue wenzhu yanjiu* (Beijing, 1988); Zhang Youyi, *Ming Qing Huizhou tudi guanxi yanjiu* (Beijing, 1984).

¹⁶ Land contracts often make note of securing permission from various kin before a land sale is allowed; in some cases, the land has first to be offered to kin before it can be sold to someone outside the surname group. While one scholar has suggested this shows the very limited nature of these land markets, he fails to consider the degree to which large kinship groups in many parts of China, especially where contracts of this sort are found, mean that a considerable portion of the relevant market for buying a parcel of land is made up of one's own kin. He also argues for the limited nature of these markets compared to European ideals as opposed to actual European practices in this period that show limited land markets. Philip C. Huang, *The Peasant Family and Rural Development in the Yangzi Delta, 1350–1988* (Stanford, Calif., 1990), 106–11.

¹⁷ The necessary data on household composition as well as land ownership and rentals for a group of families in the late imperial period to show these relationships have yet to be uncovered, but they can be seen clearly from survey data of the 1930s, in which there is no relationship between land ownership and labor power, but, due to the rental market, families balanced their cultivated land with their labor power, as reflected in a rise in the amount of cultivated land as families had more labor power. William Lavelly and R. Bin Wong, “Family Division and Mobility in North China,” *Comparative Studies in Society and History* 34, no. 3 (1992): 455–56.

¹⁸ Huang Jianhui, *Shanxi piaohao shi* (Taiyuan, 1992), 10–11.

¹⁹ Zhang Zhengming, an expert on the Shanxi merchants, places the origins of the *piaohao* in the 1820s; Zhang, *Jinshang xiangshuai shi* (Taiyuan, 1995), 119.

²⁰ Huang Jianhui, *Zhongguo yinhang yeshi* (Taiyuan, 1994), 20.

merchant group of the late imperial period and that it dates from no later than the late seventeenth century.²¹ Finally, *qianzhuang* were common in Jiangnan. More local operations, these native banks facilitated the expansion of commerce in this most wealthy of the empire's regions.²² They emerge a bit later than the native banks formed by Huizhou and Shanxi merchants. Huang Jianhui speculates that there was less need for credit in Jiangnan because goods traveled faster across shorter distances than was typical in the north.²³ Moreover, there was probably more silver in the southern half of the empire and thus possibly less need for credit mechanisms.

Although we do not yet, and may never, know as much about the operations of Chinese credit, land, and labor markets in the seventeenth and eighteenth centuries as we do about European ones, there is little doubt that these Chinese factor markets did exist. There may have been more integrated capital markets serving larger numbers of people in some parts of Europe than in much of China, and interest rates may well have been higher in China than Europe. But it is not clear how such possible differences might have mattered for economic growth—the low rates of interest in Europe may in part reflect the absence of returns to capital sufficient to raise demand that would have driven up interest rates. The more abundant evidence on product markets makes it clear that widespread commercial expansion took place in many parts of China as well as in Europe.

Together, product and factor markets formed the institutional framework within which an economic expansion based on area specialization of labor was possible. Increased labor productivity was not simply or even primarily a matter of raising the hourly or daily rates of productivity but was, more importantly, an issue of reducing the number of idle days in the annual calendar of activities. For women in Jiangnan, China's most economically developed region, increased household cotton textile production meant that they toiled at more highly paid tasks than they had before. In brief, specialization included the continued development and spread of both agricultural and handicraft technologies. Commercial expansion promoted these changes in China just as it created broadly similar developments in Europe. Of course, market institutions worked better in some areas than others, but this observation is best seen as applying to ranges of variation that existed within both China and Europe. There is no compelling evidence that the range of variation found in China was consistently inferior to that found in Europe. Efficient and inefficient marketing institutions existed in both. Economic growth in China and Europe often resulted from the development of market institutions and thus a move from less efficient to more efficient factor and product markets.

When we turn to the extension of production to new lands, there are important contrasts to be drawn between the basic features of European and Chinese imperial expansions. European imperial expansions were achieved through overseas colonies. Crossing the Atlantic, Europeans created two distinct kinds of economies, one a white settler economy in which the organizational patterns and technologies of

²¹ Huang, *Zhongguo yinhang yeshi*, 19–20.

²² Susan Mann [Jones], "Finance in Ningpo: The 'Ch'ien Chuang,' 1750–1880," in W. E. Willmott, ed., *Economic Organization in Chinese Society* (Stanford, Calif., 1972).

²³ Huang, *Zhongguo yinhang yeshi*, 33.

agricultural production largely followed those already practiced in Europe, the other an African slave plantation economy. Chinese expanded along their frontiers, first to the northwest and the southwest and later to the northeast. Along all frontiers, the common goal became the creation of sedentary Chinese agricultural and handicraft practices, which reproduced the patterns of production and exchange already found within more densely settled interior portions of the empire.

Different production patterns in China and Europe were tied to distinctive logics of exchange. In China, exchange continued to be driven by a Smithian logic of area division of labor. The newly opened frontiers had commercial ties of varying degrees of strength to more centrally located and economically productive regions. The political logic of European overseas colonies was quite different. From the vantage point of the home countries, colonies were intended to be sites where home countries could purchase goods they would otherwise have to buy from competing countries and could sell their surplus production unwanted within their immediate borders. For British colonies, which spanned the temperate zone of white settlement to the semi-tropical zone of African slave production, those colonies most closely resembling England itself were less tightly tied by trade to England than were the colonies that were most strikingly different. In the former, commercial expansion of a Smithian variety took place that made these colonies both more independent economically from the home country and more of a potential competitor.²⁴ In the latter, there was little commercial expansion within the colony of a Smithian nature; commercial ties were imposed politically through a colonial trade arrangement.

Take, for example, sugar. Sidney Mintz has stressed the ways in which plantation work discipline and production methods imposed forms of social and economic control similar to those that would later be found in the factory industry.²⁵ As with Indians working Spanish silver mines, the African slaves cannot be easily conceived of according to a Smithian market dynamic as free economic actors choosing to work on sugar plantations in order to specialize in a production that creates incomes for them to purchase marketed goods. From the New World producers' point of view, planting sugarcane did not seem part of a growing economy based on principles of comparative advantage and specialization. But when viewed from the European consumers' point of view, sugar was a commodity they could buy and use as they became more fully enmeshed in market relations.

Sixteenth and seventeenth-century Europeans began to work harder in order to purchase more commodities, including those made available by increased economic connections linking Europe to Asia and the New World.²⁶ Significantly, European per-capita incomes did not rise during this period of increased consumption of New World sugar. People engaged in commercial exchange not because it made them

²⁴ An excellent analysis of how a market economy develops in a New England setting is Winifred Barr Rothenberg, *From Market-Places to a Market Economy: The Transformation of Rural Massachusetts, 1750-1850* (Chicago, 1992).

²⁵ Sidney W. Mintz, *Sweetness and Power: The Place of Sugar in Modern History* (New York, 1985), 48-52.

²⁶ Jan de Vries, "Between Purchasing Power and the World of Goods: Understanding the Household Economy in Early Modern Europe," in John Brewer and Roy Porter, eds., *Consumption and the World of Goods* (New York, 1993), 85-132; de Vries, "The Industrial Revolution and the Industrious Revolution," *Journal of Economic History* 54, no. 2 (1994): 249-70.

better off absolutely, a normal expectation of Smithian expansion, but because they had developed preferences for goods that could only be satisfied by engaging in more market transactions, and because they would have been worse off relatively had they not engaged in these exchanges. So, from neither the perspective of the slave producer of sugar nor the European market consumer did sugar commerce seem to reflect the basic logic of Smithian exchange. The non-Smithian features of European economic expansion represented by sugar and slavery were driven by European political capacities to create new labor relations and new patterns of trade. They enhanced consumption by Europeans at the price of subjecting Africans to harsh working conditions, and fit within a European political economy of mercantilism, the Asian component of which was a kind of merchant empire that complemented European political economy in the New World and contrasted in several ways with Chinese political economy. European states and their particular kind of early modern political economy expanded their reach in ways foreign to both Chinese and other Asian practices. They made gains at the expense of others, but these politically based successes did not transform the underlying economic principles of economic growth based on commerce, specialization, and increased productivity shared by Asians and Europeans alike.

MERCANTILISM POSED A CLOSE RELATIONSHIP between power and wealth. For a state to become powerful, society had to become wealthier. This was achieved by expanding economic production in rich core areas and by extending trade across the country and especially beyond it. Merchants and rulers shared an interest in promoting national production and economic unification in order to keep wages and interest rates low and the land fully exploited. Internationally, a key goal was to build up a positive balance of trade, which would increase the domestic money supply, money being essential for prosecuting wars. Analysts treated states like individuals or firms; success was measured by spending (importing) less than one's income (exports). Rulers believed that one nation's commercial gain was achieved at another's loss. Thus competition for wealth on a global scale became a component of European state-making. European states promoted the production and commerce of their private entrepreneurs, whose successes contributed to the *consolidation and prosperity of competing states*. *Maritime expansion in particular* took place within a variety of institutional frameworks; trade was sometimes organized by government officials and in other cases by chartered companies. European rulers were anxious to protect their own merchants and pleased to gain spoils through plundering ships of other countries. What made maritime trade especially lucrative was monopoly control over some greatly desired good, such as tea or pepper. Merchants who enjoyed monopoly privileges backed by armed force to keep out competitors could make great fortunes. The neo-classical economic norm of competition among multiple buyers and sellers was in reality bounded by the two extremes of piracy and monopoly. Legally protected property rights for merchants became more secure within European countries at the same time as coercion and extraction by Europeans overseas became more widespread.

As European merchants left Europe for Asia, they discovered large and sophisticated Asian commercial networks. Just as there were various patterns of trade within Europe, so too within Asia there were many circuits of exchange. Southeast Asia, for example, imported cloth from India and silk and ceramics from China; it exported spices, woods, and rice.²⁷ Asian forms of commercial organization based on merchant communities also bore strong resemblances to European forms; Hanseatic League merchants as well as the networks of Jewish merchants in Europe each resembled in their own way the merchant communities in the Asian trading world.²⁸

The similar and parallel expansions of commerce in Asia and Europe have been a major element in broader comparisons of Asian and European patterns of historical change during the early modern era.²⁹ Amid the parallels and the similarities, we should also pay attention to differences that may prove important. We do not yet know enough about the political economies of these Southeast Asian states to make detailed comparisons with European cases, yet an initial contrast seems plausible. While many of them certainly relied on commercial revenues to fund their activities, we do not seem to find a European kind of desire for expansion based on the wedding of profit and power.³⁰ Such differences in political economy may be more helpful in explaining why Europeans sail to Southeast Asia instead of Asians to Europe than any difference in the abilities of various Asian groups to have made such journeys had they wished. For the Chinese case, differences between its political economy of agrarian empire and European merchant empire more clearly account for differences in trading practices.

The Chinese state governed an agrarian empire and did not find itself in the kind of inter-state competition that characterized European state-making dynamics.³¹ Inter-state competition in Europe stimulated economic competition as well as fiscal expansion. The economic consequences of political-economy differences were less direct and obvious than the political ones. Max Weber as well as many European

²⁷ Reid, *Southeast Asia in the Age of Commerce*, 2: 23; K. N. Chaudhuri, *Trade and Civilisation in the Indian Ocean: An Economic History from the Rise of Islam to 1750* (New York, 1985), 186–87.

²⁸ Frederic Mauro, "Merchant Communities, 1350–1750," in James D. Tracy, ed., *The Rise of Merchant Empires: Long-Distance Trade in the Early Modern World, 1350–1750* (New York, 1990), 255–86.

²⁹ Joseph Fletcher, "Integrative History: Parallels and Interconnections in the Early Modern Period, 1500–1800," *Journal of Turkish Studies* 9 (1985): 37–58, rpt. in Beatrice Forbes Manz, ed., *Studies on Chinese and Islamic Inner Asia* (Brookfield, Vt., 1995); R. Bin Wong, "China and World History," *Late Imperial China* 6, no. 2 (1985): 1–12; Jack A. Goldstone, *Revolution and Rebellion in the Early Modern World* (Berkeley, Calif., 1990); Victor Lieberman, "Transcending East-West Dichotomies: State and Culture Formation in Six Ostensibly Disparate Areas," *Modern Asian Studies* 31 (1997): 463–546.

³⁰ It is certainly true that Southeast Asian states tap an expanding commerce for revenues, but there doesn't appear to be the mercantilist logic of competition and expansion driving any of the rulers to have their merchants venture out beyond the networks of exchange that they are already establishing. For accounts of Southeast Asian states' reliance on growing cities and commercial revenues, see Reid, *Southeast Asia in the Age of Commerce*, 2: 62–131, 202–66; Peter Klein, "The China Seas and the World Economy between the Sixteenth and Nineteenth Centuries: The Changing Structures of Trade," in Carl-Ludwig Holtfrerich, ed., *Interactions in the World Economy: Perspectives from International Economic History* (New York, 1989), 72–73, offers an explanation for why the power and profit link was not forged in Southeast Asia as it had been in the Mediterranean.

³¹ See Wong, *China Transformed*, 127–51, for a longer discussion of differences in Chinese and European political economy.

specialists after him have suggested the economic importance of a competitive state system; certainly, European rulers promoted domestic economic expansion as part of their competition with others, but as we will see below, Chinese rulers also promoted domestic production and trade—their policies had larger and more positive impacts that have not yet been closely compared with European cases.

Given its large size, it is not terribly surprising to discover that the Chinese state cared much more about domestic production and commerce than it did about foreign trade. In general, the Chinese state promoted agricultural production—opened new land, repaired and expanded water control in order to expand and stabilize production of both grains and cash crops; influenced and occasionally regulated commercial distribution of some goods, most importantly grain, in order to achieve stable prices within local economies and reduce price differentials across regions; and when convinced no serious social problems between immigrants and natives would occur, encouraged migration to form new settlements so that populations and resource bases remained in relative balance. State efforts in these three general areas supported a range of economic situations, at the end points of which were two distinct types of agrarian economy: a series of small-scale, largely self-sufficient economies reproduced across an expanding empire; and a complex, large-scale interdependent economy to be monitored and if necessary managed by the state to achieve social stability. Increased production and regulated distribution could fit in either type of economy, while migration could create either new small-scale economies or frontiers to be integrated economically in a larger society. The state promoted economic prosperity through both small-scale self-sufficient and large-scale interdependent agrarian economies. Officials believed that such actions helped them gain the support of the people and thereby affirm the government's mandate to rule.

By 1500, the late imperial state possessed a complex tradition of policy options to shape economic activity. Official choices fluctuated. First, the state could choose activist and interventionist policies to control or direct economic activities; such efforts included the regulation of mining and the exchange of salt vouchers for grain shipments to troops in the northwest.³² Second, the state could satisfy itself with monitoring private sector efforts and even informally delegate responsibility or depend on others to help achieve its goals; examples include market surveillance and reliance on elites for famine relief.³³ In between the extremes of direct state control and indirect monitoring lay all sorts of efforts to redirect, channel, or limit private sector economic practices. Amid considerable variation in techniques, there was basic agreement in the eighteenth century about the type of economy that officials sought to stabilize and expand. They supported an agrarian economy in which commerce had an important role. Officials regulated salt, mining, and foreign

³² Debates on mining policies during the Qing dynasty are detailed in the archival source materials collected by Qing specialists at People's University; Zhongguo renmin daxue, ed., *Qingdai de kuangye*, 2 vols. (Beijing, 1983), 1–72. The same collection includes many documents on state capitalization of major mining operations. On salt vouchers and grain shipments to the northwest during the Ming, see Tereda Takanobu, *Sansei shōnin no kenkyū* (Tokyo, 1972), 80–119.

³³ Susan Mann, *Local Merchants and the Chinese Bureaucracy, 1750–1950* (Stanford, Calif., 1987); Pierre-Etienne Will, *Bureaucracy and Famine in Eighteenth-Century China*, Elborg Forster, trans. (Stanford, 1990).

trade. But when trade was neither foreign nor intended primarily as a revenue maker for the government, it was generally given free rein by the state, as long as officials believed that no small number of merchants were able to manipulate supplies and hence prices to the detriment of the consuming public at large.³⁴ Officials thereby supported commercial exchange without promoting concentrations of merchant wealth.

European specialists seeking a reason for China's failure to develop a modern economy speak of the state's negative attitudes toward foreign trade.³⁵ The Chinese government limited trading opportunities for foreign merchants at Chinese ports. Moreover, the Chinese government did not promote overseas exploration and merchant adventure the way Europeans had. By implication, had Chinese officials done either of the preceding, the economy would have grown more swiftly and been transformed more fully. Consider the two propositions more closely—they assume that foreign trade can help stimulate economic change. Obviously, it is not the scale of foreign trade that matters, since for a country the size of China, foreign trade can never loom large in purely quantitative terms. Rather, it is the possibility for new ideas and technologies to enter the country that is held out to have potential for the economy. But how many pre-industrial technology transfers could have had significant long-term productivity effects? It is easy though unwise to project backward the significance of technological transfers of the nineteenth and twentieth centuries to earlier conditions. This anachronistic move fails to recognize adequately the ways in which the later technological transfers involve larger amounts of information and require institutional capacities not only to absorb the initial opportunities but to take advantage of the more continuous nature of technological change in modern times. In fact, those parts of Asia first connected to Europe by merchant empires did not benefit very greatly or very swiftly from European ideas and technologies. Moreover, if the trade relations between a particular area and Europeans were constructed in an unequal way, as is sometimes argued, it becomes even less likely that such relations would encourage many positive economic changes for the Asians. Finally, the dissemination of New World food crops, arguably the technology transfer that made the largest impact on Asian economies before the nineteenth century, was not the product of exchange relations. In short, there are good reasons to be skeptical of the transformative potential of foreign trade in the early modern era.

Analysts influenced by European experiences expect states to be intimately involved with commerce, despite disagreeing about the positive or negative impact of these political activities. Within the Asian trading world, however, there was little expectation that governments would intervene on behalf of merchants. The rulers of some small states sought to control some types of trade to gain revenues, but for

³⁴ On salt, see Xu Hong, *Qingdai Lianghuai yanchang de yanjiu* (Taipei, 1972); and Chen Feng, *Qingdai yanzheng yu yanshui* (Zhongzhou, 1988); overseas foreign trade in the late imperial period is reviewed in detail by Li Jinming, *Mingdai haiwai maoyi shi* (Beijing, 1990); and Lin Renchuan, *Mingmo Qingchu siren haishang maoyi* (Shanghai, 1987). For the horse and tea trade and northwestern trade more generally, see Lin Yong and Wang Xi, *Qingdai xibei minzu maoyi shi* (Beijing, 1991).

³⁵ E. L. Jones, *The European Miracle: Environments, Economies, and Geopolitics in the History of Europe and Asia* (New York, 1981), 204, 222; John A. Hall, *Powers and Liberties: The Causes and Consequences of the Rise of the West* (Oxford, 1985), 49–50.

most merchants working in Asian port cities, there wasn't much of a home government to worry about or to depend on. Both Chinese and other traders in Asia expected little from their governments and in return did little for them.³⁶ This lack of connection between merchant and government was fundamentally altered by the European merchant empire. Irrespective of the particular institutional mechanisms deployed by a particular European country's merchants, all held a fundamental belief that state and merchant shared a common interest in exploiting economic opportunities. European notions of a merchant empire were absent from Chinese thinking not because the Chinese state was too weak to consider such an alliance between government and merchant but because it was too strong and successful in other ways to have reason to consider practices that would bring it revenue it did not anxiously desire and potential problems it did not really want. These differences clearly matter to understanding the varied political demands and possibilities of attempting to compete in an inter-state system as European states did and sustaining and even augmenting the security and stability of an agrarian empire as the Chinese state did.

HOW DO WE GAUGE the political and economic importance of this contrast between a Chinese agrarian empire and a European merchant empire? Let's consider a counterfactual in which we imagine a southeast China merchant empire being established in the seventeenth century. By tracking the plausible implications of this development, however unlikely it was itself, we can gain some sense of how the differences between merchant and agrarian empire affected later patterns of political and economic change, especially the likely political bases for increased connections between China and Europe. When the Portuguese and later the Dutch arrived in Southeast Asian ports during the sixteenth and seventeenth centuries, they found Chinese traders already present, actively engaging in both local business and long-distance trade. But unlike European merchants, these Chinese traders did not enjoy the support or act on behalf of their government's perceived interests in maritime trade. Not that the Chinese state didn't act to shape trade with foreigners according to conditions it believed would be advantageous. Witness the buildup of a military presence in the Pescadores in 1624 to force the Dutch to abandon their station and establish a base in Taiwan from which they could do business with the mainland.³⁷ Chinese officials were anxious to channel and control foreign trade, both to contain potentially disruptive activity and achieve the rents their control over trade made possible. For their part, Chinese merchants along the south and southeast coasts, together with those based in Southeast Asian trading ports, created important trading networks. Europeans increased the levels of violence in the area, stimulating the development of military capacities among both Southeast Asian rulers and Chinese merchants. At the apex of the hierarchy of successful

³⁶ Some small Southeast Asian city states depended on revenues from trade conducted at their ports, but these are not the same states as those that supplied many of the merchants.

³⁷ John E. Wills, "Maritime China from Wang Chih to Shih Lang: Themes in Peripheral History," in Jonathan D. Spence and John E. Wills, Jr., eds., *From Ming to Ch'ing: Conquest, Region, and Continuity in Seventeenth-Century China* (New Haven, Conn., 1979), 216.

Chinese merchants were individuals who combined trading and fighting with mediating skills. These figures were periodically occupied with defending themselves against the Chinese state. Labeled as a pirate when his activities were deemed illegal, a successful pirate could also be offered a position within the state's military so that his troops would immediately become imperial agents and their defeat of competitors the restoration of imperially inspired social order.³⁸ During the 1640s, when the Ming dynasty fell to the Manchus, who established the Qing dynasty, local power holders in southeast China were among the strongholds of resistance to the new conquerors. Neither the Ming state nor the elites proved able to mount a systematic and coordinated resistance for several reasons. In southeast China, perhaps the most important reason for this failure was the inability of maritime merchants to forge effective alliances with Confucian literati against the Manchus. But let's suppose for a moment that Zheng Chenggong, a major leader of anti-Manchu resistance, or someone like him, successfully resisted the Qing victory over southeast China. What would have been the consequences?

First, consider the possible consequences for China. A more independent south and southeast coast would mean weaker integration of the empire as a whole. If we assume that Zheng's empire is not broken militarily and does not self-destruct from within, the Chinese state either forges a revised relationship in which the region's de facto authority becomes recognized or it seeks to renegotiate terms of inclusion in the empire. Let's assume for the moment that whatever the relationship between this Chinese region and the capital, the imperial regime allows southeast coastal officials to promote organizationally and support militarily Chinese merchant activity in Southeast Asia, recognizing such efforts to be a strategy for seeking additional wealth and power for the regional government. What are the implications for European merchant empires?

Presumably, the Chinese can come into more direct competition with the Dutch and later the British. If the Chinese are subsequently defeated either militarily or economically, then little is altered. But, if instead, the Chinese succeed in establishing their superiority militarily and economically, we would expect them to sustain control over larger portions of intra-Asian shipping and perhaps even begin to participate in the trade to Europe. Chinese merchants could, in an extreme scenario, become representatives of a political economy as commercially aggressive and militarily secure as any European power's. If the south and southeastern coastal Chinese developed the military and economic capacities to compete with Europeans in the eighteenth century, this could have mattered for nineteenth-century developments. We could of course easily doubt the significance of this hypothetical eighteenth-century parity, knowing that the changes in European naval armaments opened up a considerable gap between European offensive capacities and Chinese defensive capacities in the 1830s and 1840s. But we might alternatively imagine the Chinese state buying the technologies and importing the military hardware necessary to make defense more feasible. Using tea and silk revenues, the Chinese had the foreign exchange to make such purchases conceptually feasible in economic terms. Politically, British demands to import Indian opium in ever larger quantities

³⁸ Li Jinming, *Mingdai haiwai maoyi shi*, 80–108, 173–83; Lin Renchuan, *Mingmo Qingchu siren haishang maoyi*, 40–84.

could conceivably have been less pressing because the trade balance would have been changed by military imports to China. If the British were less pressed to send opium to China and the Chinese were sufficiently strong to withstand the diminished pressures to allow additional opium into the country, the silver outflow would not have taken place with its attendant negative impacts of deflation on the commercial economy. A Chinese rebuff of British insistence on their right to sell opium would have placed political constraints on the commodities considered morally acceptable for trade. It could also have meant a Chinese capacity to refuse the construction of treaty ports in China, a symbol of Chinese subordination to European power. In short, a politically powerful China more able to resist militarily the British demands of the 1830s and 1840s could have resulted from a successful southeast Chinese merchant empire. Farfetched no doubt, but specifying how a change in eighteenth-century Chinese political economy could have had far-reaching consequences in the nineteenth century helps us to set bounds on what was more likely and reasonable.

The kind of economic exchange posited toward the end of this counterfactual bears underlining because it is of a kind that had not yet become important in Asia's relationship to Europe: the exchange of Chinese primary goods and luxury items for European industrial goods. China's lack of demand in the early nineteenth century for British and other European goods is well known. To undermine the argument that the Europeans produced nothing the Chinese did not already produce cheaply enough on their own, we have to allow for that new set of goods made possible by the Industrial Revolution, not the textiles for which there were longstanding substitutes but the iron and steel armaments with which the British battered down Chinese defenses. We furthermore need to imagine the economic and political leaders of both Chinese and European political economies in the 1820s and 1830s being able to agree to the transfer of these new technologies to China. Without these kinds of exchange—Chinese tea and silk for European armaments—it is difficult to see what kinds of exchange between Chinese and European products would have taken place for purely economic reasons.³⁹ Smithian dynamics of market integration were not strong enough, without the industrial differential, to promote the integration of Chinese and European economic activities. In other words, the basic economic dynamics of Smithian growth we can see taking place in various parts of Asia and Europe before the nineteenth century do little to link economically Asia and Europe together. I have already noted that the links that European political economies forged with Africa and the New World before the nineteenth century were not of a Smithian variety. What, then, was the nature of the trade connections between Asia and Europe in this period?

THE EUROPEANS WHO BUILT MERCHANT EMPIRES IN ASIA competed for control over goods they shipped to Europe and became involved, when they could compete, in

³⁹ My colleague Kenneth Pomeranz has suggested to me that we could imagine Indian cotton and indigo being exchanged for Chinese tea and silk, but this still would not be purely market-driven trade between Chinese and *European* products since British manipulation of Indian commodities would once again introduce political factors.

intra-Asian networks of exchange. Their economic success against each other was influenced by the ways they organized their trading institutions. More generally, their economic presence in Asia depended on two skills, a capacity to understand and work with Asian mercantile customs and a military capacity to promote their presence in order to establish new trading connections between Asia and Europe. Unmistakable European hegemony in East Asia does not emerge until the nineteenth century, when westerners can impose their vision of international relations and international trade. The successes that some Europeans achieved in the seventeenth century as merchants engaged in intra-Asian trade, notably the Dutch working the routes between coastal China and Japan, were eclipsed in the eighteenth century when a round of Chinese-based commercial expansion tied the Southeast Asian economies to Chinese growth.⁴⁰ Even when Europe established a political and economic power in nineteenth-century Asia, the dynamics of economic and political change within the East Asian region can still be understood largely in terms of economic and political relations internal to the region. Thus Kawakatsu Heita has proposed explaining nineteenth-century Japanese industrialization not in terms of a response to threats from Great Britain and continental Europe but in terms of Japan's competition with China for economic and political centrality within East Asia.⁴¹ European inventions and innovations certainly matter to Chinese and Japanese in the nineteenth century, but even as European powers are able to impose unequal treaties on the Chinese and for a briefer period on the Japanese, they are unable to define stable roles for the economies of these countries within their world economy. Understanding how these economies change between the mid-nineteenth and mid-twentieth centuries depends at least as much on understanding economic and political relations in Asia as it does on global connections.⁴²

Two further features of the connections forged between European traders and the Asian trading world deserve stress. They place in a broader context the way in which the new institutional economics has explained the success of the Dutch and British over the Spanish and Portuguese in expanding their commercial spans in terms of how they organized their firms. The Spanish and Portuguese had government-centered monopolies seeking to make monies off their control of goods, while the Dutch and British set up companies that were given greater freedom to establish their commercial practices.⁴³ The contrast is useful but incomplete in two ways. First, military power clearly mattered to determining what

⁴⁰ Klein, "China Seas and the World Economy between the Sixteenth and Nineteenth Centuries," 83–86.

⁴¹ Kawakatsu Heita, "Nihon no kōgyōka o meguru gaiatsu to Ajia ken kyōsō," in Hamashita Takeshi and Kawakatsu Heita, eds., *Ajia kōeki ken to Nihon kōgyōka 1500–1900* (Tokyo, 1991); Heita Kawakatsu, "The Emergence of a Market for Cotton Goods in East Asia in the Early Modern Period," in A. J. H. Latham and Heita Kawakatsu, eds., *Japanese Industrialization and the Asian Economy* (London, 1994), 9–34.

⁴² Peter Schran, for example, has argued that Japanese imperialism defined the consumer and capital markets transforming Asia's economy in the late nineteenth and early twentieth centuries; see Schran, "Japan's East Asia Market, 1870–1940," in Latham and Kawakatsu, *Japanese Industrialization and the Asian Economy*, 201–38.

⁴³ Douglass C. North, "Institutions, Transaction Costs, and the Rise of Merchant Empires," in James D. Tracy, ed., *The Political Economy of Merchant Empires* (New York, 1991), 22–40; North makes a more general argument about the nature of property rights determining differential growth

group of merchants was able to establish a strong market position in at least certain products in Asia. Europeans brought to their battles a different calculus and a greater intensity that often led them to establish a military superiority when they engaged others. Both the Portuguese and then the Dutch established fortified ports with massive moats and walls, which proved impregnable to attacks launched by Muslim leaders seeking to keep the European intruders from establishing their militarized presence.⁴⁴ Second, European commercial practices were in some ways similar to Asian ones, and when they differed, European trading institutions were not clearly superior. If they were, we would expect to see one of two outcomes to have been more frequently observed: either Asians would have adopted European practices or Europeans would have displaced Asian traders; neither occurred. Asian traders, for instance, continued to occupy positions of prominence through Southeast Asian ports in the nineteenth century, relying on a combination of personal ties, oral contracts, and their own financial networks to organize trade. European traders remained a minority presence along some of the trade routes; for instance, as late as 1850 European traders in north Java ports accounted for less than 20 percent of total tonnage, down from 38 percent in 1840 and 41 percent in 1830.⁴⁵

The persistence of Asian traders in the Asian trading world meant that there was no single institutional form for firms that was uniformly superior to all others. Instead, different kinds of commercial practices could coexist economically. The relative advantages of different trading practices and commercial institutions could be very modest. This reality reinforces the small band of variation in levels of production and rates of productivity that characterized early modern economies generally. It prepares us to wonder more about the nature of the trade that connected East Asia and Europe. Can Smithian market dynamics explain why early modern trade takes place between these two regions of the world? Can the political logic of colonial trade provide an alternative explanation of trade relations? Neither is an adequate guide. To understand trade relations between East Asia and Europe, we have to look more at the role of silver.

Within early modern Europe, silver currencies as well as other monies were used to settle accounts across borders and thus equilibrate any imbalances in real trade. But the situation changes once we go beyond Europe. Silver from the New World did flow to Europe and beyond in order to balance commodity trade flows between the New World and other parts of the world. The Spanish controlled most of the silver and brought it to Europe to pay for goods and services; other Europeans used silver to buy goods within Europe and from beyond. But once the silver was used to purchase commodities in Asia, it cannot be considered as a transfer of currency in modern international trade terms, since we would expect a change in the relative price levels in the domestic economies from which silver was sent and those into which it flowed. Instead, we have to treat the silver flowing to Asia as a commodity

rates among European countries in Douglass C. North, *Structure and Change in Economic History* (New York, 1981), 147–57.

⁴⁴ Geoffrey Parker, "Europe and the Wider World, 1500–1700: The Military Balance," in Tracy, *Political Economy of Merchant Empires*, 169.

⁴⁵ Denys Lombard, *Le carrefour javanais: Essai d'histoire globale*, 3 vols. (Paris, 1990), 2: 87.

for which various Asian economies, especially China's, exerted a strong demand.⁴⁶ Asian countries in the sixteenth and seventeenth centuries paid for this silver by sending other commodities—silks, ceramics, cotton textiles, spices—to Europe. But it isn't as helpful to think of silver as simply a commodity once it enters Asian economies, since it is used as currency. Because the transfers of silver do not affect relative prices, and hence trade flows between Asia and Europe as it would if silver were a currency affecting relative price levels, it appears that the currency systems of Europe and Asia remain quite independent in the early modern period.

Not all Europeans watching the outflow of silver to Asia believed they were simply trading silver as a commodity for other goods. Mercantilist thinking had taught them to believe that a country prospered when it sold more of its goods than neighboring countries and when it maintained a positive balance of trade. Exports of silver created difficulties for this perspective. As long as European mercantilists understood themselves to be competing with each other and not with Asian governments, the constant flows of silver to Asia could be disquieting to some but acceptable more generally. Anxieties grew more serious in the late eighteenth century, when mercantilist thinking became less dominant, and British merchants and the government increasingly saw the outflow of silver as a problem of unbalanced trade. They looked for and found a mechanism that could avoid an outflow of silver.

The British solution was opium. Indian-grown opium was sold to the Chinese. In the latter part of the eighteenth century, opium imports began to balance Chinese exports of silks, ceramics, and teas. By the 1820s and 1830s, the increased imports of opium led to a silver outflow from China and from 1839 to 1842 a war between China and Great Britain. Through the eighteenth century, the trade linking Britain, India, and China formed two sides of a triangle: opium and raw cotton flowed from India to China and tea as well as silk and ceramics went from China to Britain. By the mid-nineteenth century, all sides of the triangle were filled in: opium and raw cotton continued to flow from India to China, while Chinese exports to Britain were augmented by a silver outflow; for its part, Britain sent cotton textiles to India, having labored successfully to destroy the Indian handicraft textile industry, which had been the world's major exporter of cotton textiles and thus Britain's major

⁴⁶ Recent arguments to treat the silver going to Asia as a commodity include Dennis O. Flynn and Arturo Giráldez, "Born with a 'Silver Spoon': The Origin of World Trade," *Journal of World History* 6, no. 2 (1995): 201–22; Flynn and Giráldez, "Arbitrage, China, and World Trade in the Early Modern Period," *Journal of the Economic and Social History of the Orient* 38, no. 4 (1995): 429–48; Richard von Glahn, *Fountain of Fortune: Money and Monetary Policy in China, 1000–1700* (Berkeley, Calif., 1996), 113–41; Andre Gunder Frank, *ReOrient: Global Economy in the Asian Age* (Berkeley, 1998), 131–50. The degree to which the world of this period should be viewed to have an integrated world economy varies among these scholars, with von Glahn offering the most cautions and Frank proclaiming the strongest connections; see also William Atwell, "Ming China and the Emerging World Economy, c. 1470–1650," in Denis Twitchett and Frederick W. Mote, *Cambridge History of China*, Vol. 8: *The Ming Dynasty, 1368–1644, Part 2* (Cambridge, 1998), for a view of China's increasing connections with an international economy through the silver trade without an explicit discussion of silver as a commodity. For a more skeptical view of Chinese market integration domestically and internationally, see Kuroda Akinobu, *Chūka teikoku no kozo to sekai keizai* (Nagoya, 1994). Kuroda contrasts China's eighteenth-century domestic and foreign trades with those of the late nineteenth century and suggests that in the eighteenth century neither domestic nor foreign trade was well integrated, the situation being very different from the late nineteenth century, when more signs of market integration both domestically and internationally were present.

competitor.⁴⁷ Opium replaced two sides of an earlier triangle: New World silver to Europe and then Asia, with Asian products being sent to Europe. Neither of these incomplete triangles in which either silver or opium goes to China in exchange for goods sent to Europe conforms to the kinds of trade exchange more typical in the later nineteenth and twentieth centuries.

There do not appear to be any products for which Smithian dynamics alone could create long-distance trade spanning the oceans before the nineteenth century. Asian spices, teas, porcelains, and textiles made their way to Europe in the early modern period, but there were few goods produced by Europeans that were enjoying much demand in Asia. Instead, Asians bought the silver that initially came from Spanish-controlled New World mines. While silver can be thought of as a commodity as it moves between the New World, Europe, and Asia, it is not easy to think of it as a commodity in a Smithian exchange based on specialized division of labor. Asian producers were paying for their currency that originated in the New World by exporting goods to Europe. The idea of global trade being at its base driven by one regional economy's demand for currency is a situation quite different from how global trade works in more recent times, when we expect Smithian dynamics to play more of a direct role.

Of course, Adam Smith himself did not think of international trade as necessarily taking place according to some basic division of labor. In fact, he believed, as did many of his contemporaries, that foreign trade was "vent-for-surplus."⁴⁸ *Countries sold surplus commodities in order to gain other goods.* For their part, mercantilist thinkers believed that a foreign trade surplus brought in bullion, which was useful because it helped generate additional domestic economic activity, a virtue analytically distinct from the great desire of rulers to gather monies needed to make wars. The mercantilist preference for trade surpluses makes sense because an increased money supply attending trade surpluses provides the funds to stimulate additional employment and use of resources. As long as an economy expands to absorb the additional silver, in large measure through expanding commerce, then the mercantilist preference for trade surpluses makes sense. Trade flows of this sort tend to promote the wealth and expansion of the surplus-producing country compared to its neighbors. Among European countries, Great Britain had been a most successful mercantilist power, expanding its productive capacities more swiftly than its competitors. With these advantages and driven by a desire to open even larger markets, British political economists, most famously Adam Smith, began arguing in the late eighteenth century for free trade. Britain's early modern economic successes seem to be the groundwork for later achievements.

But wait. If assembling trade surpluses to gather money for domestic commercial expansion is a sign of successful mercantilist efforts, then China, and not Britain, may have been the most successful. Estimates vary, but if more than 7,000 and less than 11,000 metric tons of silver entered China from European and

⁴⁷ Chen Ziyu, "Yi Zhong Yin Ying sanjiao maoyi wei zhou tantao shijiu shiji Zhongguo de dui wai maoyi," in *Zhongguo haiyang fazhanshi lunwenji bian yi weiyuan hui zhubian*, eds., *Zhongguo haiyang fazhan shi lunwenji*, vol. 1 (Taipei, 1984).

⁴⁸ Michael Hudson, *Trade, Development and Foreign Debt: A History of Theories of Polarisation and Convergence in the International Economy* (London, 1992), 41.

Japanese sources between 1550 and 1645, China, more than any parts of Europe, likely used more silver effectively to promote commercial expansion broadly.⁴⁹ To suggest that the Chinese economy succeeded at the mercantilist's game of accumulating silver in order to promote commercial expansion does not mean that Chinese policymakers deliberately set out to achieve this end. While some officials supported the influx of silver into the country, others voiced multiple objections: some worried that there wasn't enough silver to serve their economy's needs for money; others disliked dependence on foreign sources for their money supplies. These concerns were tied to beliefs that silver favored large-scale, long-distance trade to the detriment of more local levels of commerce for which copper cash was more frequently employed. Opposition to silver use was also connected to critiques of centralized government, which was seen to display a voracious appetite for silver since agricultural taxes, the main source of state revenue, had been commuted from in-kind payments to silver.⁵⁰

The Chinese political economy was not "mercantilist" since its policymakers did not conceive of themselves as competing for wealth and power with other like-minded governments. But from a mercantilist perspective, the flows of silver to China represented the success of the Chinese economy at a European game. From a European perspective, Britain's success at mercantilist expansion can be seen to set the conditions for a shift to free trade philosophy and Britain's continued economic preeminence among European countries. From a comparative perspective embracing both China and Europe, however, the logic of the British shift from mercantilism to free trade is no longer as obvious or simple. If the Chinese were successful at this particular mercantilist game of attracting silver bullion, why were they unable to sustain their competitive position when the British shifted from mercantilism to a regime of free trade? The answer, in part, must include a recognition that success at mercantilist goals did not lead naturally and necessarily to an embrace of free trade or to success at free trade. The basic nature of the international trade between Asia and Europe changed in the nineteenth century as the underlying dynamics of economic expansion were transformed. The basic European logics of colonial trade, vent-of-surplus, and the accumulation of silver bullion became far less important in a nineteenth-century economic world reconfigured by the Industrial Revolution.

THE GAP BETWEEN SILVER'S ROLE in the international trade of the early modern world and the role of money in modern international trade is represented by the failure of early modern trade to conform to the norms of international monetary theory of more recent times. In an idealized scenario we associate first with David Hume, flows of money in modern international trade balance differences between a country's imports and exports. Prices in the country receiving more currency rise, while those in the country sending out currency in principle fall. These adjustments in relative prices make the goods of the country sending out currency more

⁴⁹ The estimates are figures rounded to the nearest thousand from von Glahn, *Fountain of Fortune*, 140.

⁵⁰ Von Glahn, *Fountain of Fortune*, 142–224.

competitive in international trade at the same time as the country receiving additional currency sees its domestic prices rise and its goods become less attractive on international markets. At some point, we can imagine the relative prices across countries reaching an equilibrium so that currency transfers are no longer necessary. Specialization and division of labor across countries means that people in the international market are all producing those goods for which they are best suited in terms of comparative advantage. Such an ideal and imaginary point is conceivable only in a fixed or static environment. Once we allow for changes over time, trade flows will once again become unbalanced; most importantly, new countries enter into trading networks or begin producing new commodities that compete with those of other countries, while some countries do better than others at raising productivity levels.

In reality, international trade is part of more complex and varied scenarios. Its nineteenth-century role in facilitating or limiting economic development seems to vary among different cases. At one extreme are those areas that become raw materials producers. Such areas have a clear antecedent in the early modern era when economically and politically powerful European countries sought to make their colonies into exclusive trading partners according to a principle of economic complementarity in which the metropolitan country would export manufactured goods and the colony raw materials.⁵¹ The division of activity was meant to preclude competition between the two and was intended to profit the metropole at the expense of the colony. Some scholars argue that nineteenth-century international trade continues along the axis connecting raw materials producers and industrial centers, even when the political framework of colonialism is replaced by a free trade system. This is at the heart of the world-system analyses inspired by Immanuel Wallerstein's work. Capitalism creates a worldwide division of labor that makes difficult, if not impossible, the development of many non-European parts of the world. But there are clearly sites beyond Europe that do develop economically. At this other extreme are nineteenth-century countries that engaged in international trade and, through these relations, developed the capacities to industrialize as they gained some combination of new technologies and institutions, as well as the industrial raw materials and capital necessary to begin a process of economic transformation. The most obvious cases are in North America, where both the United States and Canada became major agricultural exporters during the second half of the nineteenth century and from these bases begin to industrialize. Japan offers a second kind of case where investment in a range of industries during the late nineteenth century led to the creation of expanded production for markets in Japan and in Asia.

Modern world-system analyses underscore the structural inequalities in economic relations. Economies diverge in their development possibilities. Conventional economic theory argues to the contrary that convergence is the norm, with market integration creating similar opportunities for development across ever broader expanses. Despite fundamental disagreements over how to assess the nineteenth-century international economy, both approaches realize that there were

⁵¹ Richard Anthony Johns, *Colonial Trade and International Exchange: The Transition from Autarky to International Trade* (London, 1988), 49–64.

clearly more kinds of linkages forged than had existed in early modern times. Major international flows of agricultural goods began to crisscross different parts of the globe. Transportation advances, railroads and steamships especially, made possible the movement of raw materials and finished goods over great distances to connect the economic labors of increasing numbers of people. Whether we speak of late nineteenth-century Southeast Asian rubber or twentieth-century Middle Eastern oil, raw material and resource flows move on a scale and in volumes beyond what was conceivable in the early modern era when there weren't the shorter routes made possible by the Suez (1869) and Panama (1914) canals or the swifter modes for transporting raw materials and finished goods. These movements become a basic feature of specialization and division of labor that makes for a kind of economic integration not part of the early modern era. Production processes before the nineteenth century did not routinely depend on movements of raw materials over long distances. Only an industrial capitalism could create the levels of productivity to warrant the concentration of resources and labor at a limited number of sites to manufacture a range of goods to be used across the world's continents. The process began before the major nineteenth-century and early twentieth-century transportation advances when British capitalists purchased American cotton to supply their textile mills, but the degree to which industrial capitalism could integrate distant areas increased dramatically once transportation and communication technologies advanced.

Under nineteenth-century industrial capitalism, the radical increase of resource and raw material movements was complemented by growing flows of capital channeled through an expanding set of financial institutions that created new methods for moving funds. Not surprisingly, those parts of the world with less developed financial institutions were more vulnerable to monetary instabilities.⁵² Western flows of capital to Asia, for instance, financed plantation enterprises, textile mills, steamships and railroads. Creating new centers of industrial production as well as connecting raw material suppliers to industrial producers, increasingly global flows of capital created kinds of economic connections largely absent in the early modern era. Similarly, the nineteenth and twentieth centuries witnessed increased migration. Many of these movements, including nineteenth-century European migrations to the United States and Chinese migrations to Southeast Asia and the Americas have had a strong economic stimulus, based on varying combinations of economic push from difficult conditions and the economic pull of opportunities in another setting. From an economic perspective, movements of labor, like those of resources and capital, reinforced the increasing density of economic connections across countries, continents, and oceans. With markets for capital and labor connecting distant parts of the globe, the world became more integrated economically than it had been in early modern times. While there remained a considerable gap between neo-classical ideals of markets and the realities of varying degrees of market integration for goods and factors of production, it became plausible for economists to imagine a world in which resources and products could flow over long distances under a regime of free trade.

⁵² Barry Eichengreen, *Globalizing Capital: A History of the International Monetary System* (Princeton, N.J., 1996), 39–41.

It would have been far more difficult to develop such ideas under early modern conditions.

The institutions and practices of the international economy of the late nineteenth and twentieth centuries are well known. Also well accepted is the premise that nineteenth-century industrial capitalism promoted new forms of economic growth and concentrations of economic wealth, however much disagreement remains over the levels and rates of economic convergence or divergence during this period. There remains the huge and complex question of accounting for the move from the economic and political world of the early eighteenth century to that which clearly had emerged by the mid-nineteenth century. This article has not aimed to address this problem directly. But if its suggestions about Smithian dynamics in the early modern era are reasonable, namely that broadly similar Smithian dynamics accounted for much of the economic expansion across Asia and Europe and that these dynamics did little to integrate economic activities in Asia with those in Europe, then neither David Landes's recent efforts to celebrate European distinctiveness nor Andre Gunder Frank's equally vigorous labors to assert Asian superiorities in a global economy quite hit the target in their sweeping accounts of economic change since the early modern era.⁵³ The former repeatedly imagines differences, frequently of a cultural nature, between Asia and Europe the significance of which are not made plain, while the latter often labels as economic connections between Asia and Europe what are more certainly similar and even parallel dynamics of economic development. Unless the nature of similarities in the economic dynamics of growth and the differences among Asian and European political economies in the early modern era are joined to an appreciation of differences in the nature of international trade before and after the advent of industrial capitalism, we will not be able to pose clearly the kind of explanatory problems we face in accounting for changing dynamics of economic change between the early modern and modern eras.

AS SCHOLARS HAVE LONG KNOWN, regions of the early modern economic world became more connected to each other than they had been before 1500. At the same time, these regions were more similar in terms of the dynamics promoting economic growth and expansion than previously believed. There were important differences between the political economies of agrarian and merchant empire, which help explain why it is that certain European powers established their presence in both Asia and the New World rather than other Asian or American powers having done so in Europe. But the aggressiveness of Europeans does not mean they were necessarily the most economically successful powers of the era. Indeed, by the mercantilist criterion of amassing silver bullion to promote domestic commercial expansion, China did more than much of Europe. Early modern maritime trade obeyed several economic and political logics that collectively formed a set of relations quite different from those present under nineteenth and twentieth-century

⁵³ David S. Landes, *The Wealth and Poverty of Nations: Why Some Are So Rich and Some So Poor* (New York, 1998); Frank, *ReOrient*.

conditions. The denser weave of global connections possible under industrial capitalism depended crucially on the cluster of scientific and technical changes that established Europeans in positions of political and economic ascendance they did not enjoy before the nineteenth century. Early modern European economic institutions had not been designed in order to create industrialization, but they clearly mattered in two distinct ways once industrialization became possible. First, European institutions often worked well to create industrial growth; for example, financial networks facilitated larger-scale mobilizations of capital, and legal systems assured more complex forms of contracting. Second, European ways of conducting business set standards with which others had to conform if they expected to become part of an international economy, not necessarily because these methods were universally superior to those used elsewhere but because integration increasingly required the adoption of consistent and complementary practices. Early modern logics of Smithian commercial expansion were still very important; in fact, they could operate more effectively over longer distances than before. Early modern strategies of imposing politically divisions of labor continued under different guises in later centuries as well. But these continuities take on new meanings under industrial capitalism, which redefines what international trade can be about and in the process creates both new economic problems and possibilities for the world's population—problems and possibilities we continue to live with today.

The differences between the early modern and modern economic systems offer an important vantage point from which to perceive what did and did not distinguish European from Asian economies before the nineteenth century. If the more successful parts of Asia and Europe were not so very different in terms of their successes or the limitations of those successes, then we can pinpoint more closely the foundations and extent of Western hegemony in the nineteenth and twentieth centuries. For the moment at least, it appears that there were more economic similarities than differences across Eurasia before 1800 and that the connections of the early modern era, while more salient than those of the centuries preceding it, were far less significant economically than those that followed under industrial capitalism, when a shifting economic central core in the West was able to assert a global dominance that catapulted first northwestern Europe and then the United States into positions of power and authority they would enjoy through the twentieth century and into the twenty-first.

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Modern Inequality and Early Modernity:
A Comment for the *AHR* on Articles by
R. Bin Wong and Kenneth Pomeranz

DAVID LUDDEN

EUROPE MADE MODERNITY BOTH UNIVERSAL and uniquely European.¹ Kenneth Pomeranz and R. Bin Wong tackle one part of that invention, the idea that Europe contained a unique, indigenous capacity to industrialize that made it a universal model for economic development.² In the light of this idea, economic disparities among rich and poor countries seem to emerge because nations succeed variously in developing their own style of Western capitalism, as North America did most successfully. Europe's modernity seems to be the secret of global success: the more integrated into the world economy any country is, the richer it becomes, as economies around the world converge along a trend of increasing productivity and material well-being in a historical process that is now called globalization. This entire scenario is in question, and these authors reconsider its original validity.³

Pomeranz and Wong accept the idea that industrialization launches modern economic development and ask why it happened first in Europe and not China, specifically, in Britain but not the Yangzi Delta. Scholars have answered this question in the past by saying that no matter how rich China had once been, it was not ready for industrialization in 1800.⁴ Pomeranz and Wong conclude, however, that parts of China were indeed ready, which reopens the question, "Why Europe?" They argue that Europe's modern productivity did not arise from uniquely European modes of economic organization or from uniquely European levels or patterns of economic growth in early modern centuries. British industrialism depended on British natural resources and British assets in the wider world. It

¹ Dipesh Chakrabarty, *Provincializing Europe: Postcolonial Thought and Historical Difference* (Princeton, N.J., 2000).

² See also Kenneth Pomeranz, *The Great Divergence: China, Europe, and the Making of the Modern World Economy* (Princeton, N.J., 2000); R. Bin Wong, *China Transformed: Historical Change and the Limits of the European Experience* (Ithaca, N.Y., 1997).

³ The best guide to the literature is John Martinussen, *Society, State, and Market: A Guide to Competing Theories of Development* (London and Dhaka, 1997). On economists' new thinking about convergence and divergence in light of new data, see <http://www.worldbank.org/poverty/inequal/econ/intro.htm>.

⁴ Mark Elvin, *The Pattern of the Chinese Past* (London, 1973); Philip C. Huang, *The Peasant Family and Rural Development in the Yangzi Delta, 1350–1988* (Stanford, Calif., 1990); Susan Naquin and Evelyn S. Rawski, *Chinese Society in the Eighteenth Century* (New Haven, Conn., 1987).

occurred during a particular conjuncture in world history and could have happened elsewhere.

This work sheds new critical light on modernity by detaching its origins from Europe and by arguing that early modernity was not modernity's logical precursor or inchoate immanence. Transitions followed unpredictable pathways. Contingency and conjuncture shunted China and Europe into discontinuous trends that Pomeranz calls "the great divergence." As Wong indicates, exogenous factors affecting China and Europe moved inside networks of mobility that carried people, ideas, armies, commodities, technology, and finance all over the early modern world. These networks had a broad impact on regional histories in Europe, Africa, America, and Asia.⁵ Networks that connected regions of early modern China, each with their own individual histories, also ran into Central Asia and overseas.⁶ Europe had separate but connected regional histories that spilled outside Europe. Activities in and across the Atlantic Basin, Indian Ocean, and Pacific Rim participate in both European and Chinese history. Early modern spaces did not enclose historical territories in the manner of modern national boundaries. In the course of "the great divergence," modernity disciplined the historical imagination inside national state territories, so that pre-modern histories appeared to operate inside the same national confinement.⁷

Pomeranz and Wong weave histories of China and Europe together in innovative and challenging ways. Changing times are changing historical thinking about modernity. Here we have two historians of China, teaching in North America, writing for an American journal, doubly transgressing boundaries of European history, first by comparing Europe to China, and then by saying that Europe's take-off to modern economic growth⁸ was contingent, conjunctural, and partly exogenous. These ideas seem to be apt for the history of an age before modern boundaries arrived, and for scholars in a day when modern boundaries are dissolving, but transgressing national histories presents empirical, conceptual, and diplomatic problems for a profession built on national sensibilities. My comments here focus, therefore, on the importance of "the great divergence" as part of a larger historical process of modern inequality that runs across all national histories and into the intimate nooks and crannies of everyday localities on all the continents.

⁵ See Sanjay Subrahmanyam, "Connected Histories: Notes toward a Reconfiguration of Early Modern Eurasia," *Modern Asian Studies* 31 (1997): 735–62; David A. Washbrook, "South Asia, the World System and World Capitalism," *Journal of Asian Studies* 49, no. 3 (1990): 479–508; John Lee, "Trade and Economy in Pre-industrial East Asia, c. 1500–1800: East Asia in the Age of Global Integration," *Journal of Asian Studies* 58, no. 1 (1999): 2–26; Dennis O. Flynn, "Comparing the Tokugawa (sic) Shogunate with Hapsburg Spain: Two Silver-Based Empires in a Global Setting," in *The Political Economy of Merchant Empires*, James D. Tracy, ed. (Cambridge, 1991); Dennis O. Flynn and Arturo Giráldez, "Born with a 'Silver Spoon': The Origin of World Trade in 1571," *Journal of World History* 6, no. 2 (1995): 201–21.

⁶ William S. Atwell, "International Bullion Flows and the Chinese Economy circa 1530–1650," *Past and Present* 95 (1982): 68–90; and Atwell, "Ming China and the Emerging World Economy, c. 1470–1650," in *The Cambridge History of China*, Vol. 8: *The Ming Dynasty, 1368–1644*, Part 2, Denis Twitchett and Frederick W. Mote, eds. (Cambridge, 1998).

⁷ Kären Wigen, "The Geographic Imagination in Early Modern Japanese History: Retrospect and Prospect," *Journal of Asian Studies* 51, no. 1 (1992): 3–29; and Wigen, *The Making of a Japanese Periphery, 1750–1920* (Berkeley, Calif., 1995).

⁸ On "take-off" modeled on Europe and theorized as a general process, see *The Economics of Take-Off into Sustained Growth*, W. W. Rostow, ed. (New York, 1968).

Pomeranz and Wong describe an early modern baseline from which to measure economic inequality between Europe and China. They see Europe and China as having similar potentials for economic development in the eighteenth century. They argue that the great economic divergence of the nineteenth century defies explanation without taking into account exogenous assets that sustained Britain's original industrialization. These assets were exogenous in two senses: some lay outside Britain's national borders and others came from outside the market economy. Britain acquired coal, cotton, and profitable trade opportunities around the world by means other than "Smithian" market exchange. These assets gave Britain economic advantages China did not have. Britain used these advantages to fuel a modern industrial development that spread across Europe and North America, as Western countries enriched one another with industry (and, we might add, with many assets acquired by non-Smithian means, including all the natural resources in the Americas).

Early modern baseline similarities with Britain also characterized regions in South Asia, where merchants and manufacturers communicated and competed in networks of mobility that walked, ran, sailed, and then steamed from one end of Eurasia to the other.⁹ As Wong points out, similarities among regions across Eurasia continued after divergence began. Industry actually spread faster in parts of Asia than in much of Europe and America. The first Indian cotton mill appeared in Bombay in 1853, and in 1881, the British government imposed the first Factory Act to counteract India's competitive advantage from low-cost labor and raw materials. In 1914, India had the world's fourth largest cotton textile industry. Textiles, coal, iron, steel, jute, and other industrial manufactures comprised 20 percent of Indian exports, valued at 10 percent of national income.¹⁰ The American consul at Bombay then called India "one of the few large countries of the world where there is an 'open door' for the trade of all countries."¹¹ In the next twenty-five years, Indian industrialization increased 58 percent, compared to slower, more uneven growth in Europe.¹² Nevertheless, India's work force became increasingly agricultural and its population more subject to chronic hunger, in an open economy constructed on European lines, where most new farm investment benefited commercial crops destined for export.¹³ In 1943–1944, famine in Bengal—provoked by world war and spread by ubiquitous market forces—killed three million people in one year.¹⁴

Economic divergence thus involved more than rates of industrialization, and

⁹ Andre Gunder Frank, *ReOrient: Global Economy in the Asian Age* (Berkeley, Calif., 1998); K. N. Chaudhuri, *Asia before Europe: Economy and Civilisation of the Indian Ocean from the Rise of Islam to 1750* (Cambridge, 1990); Prasannan Parthasarathi, *The Transition to a Colonial Economy: Weavers, Merchants, and Kings in South India, 1720–1800* (Cambridge, 2001).

¹⁰ *Inland Trade (Rail and River-borne) of India, 1919–1920* (Calcutta, 1921); David Ludden, "Development in South Asia," in *International Encyclopedia of the Social and Behavioral Sciences*, Neil J. Smelser and Paul B. Baltes, editors-in-chief, 26 vols. (Amsterdam, 2001).

¹¹ *British India, with Notes on Ceylon, Afghanistan, and Tibet*, U.S. Department of Commerce, Special Consular Reports, No. 72 (Washington, D.C., 1915), 9.

¹² Morris D. Morris, "The Growth of Large-Scale Industry to 1947," in *The Cambridge Economic History of India*, Vol. 2, c. 1757–c. 1970, Dharma Kumar, ed. (Cambridge, 1983), 569, 576, 609.

¹³ The best short histories of economic development in South Asia are B. R. Tomlinson, *The Economy of Modern India, 1860–1970* (*The New Cambridge History of India*, Vol. 3, Part 3) (Cambridge, 1998); and Tirthankar Roy, *The Economic History of India, 1857–1947* (Delhi, 2000).

¹⁴ Amartya Sen, *Poverty and Famines: An Essay on Entitlement and Deprivation* (Oxford, 1981), 1–86.

indeed, more than economics. There were no statistics to measure diverging national incomes before 1870, but already by 1820, intellectuals in London and Calcutta claimed that Britain led the way to modernity.¹⁵ This claim did not arise from economic analysis as much as from the cultural impact of British military victories over Napoleon, Gurkhas (in Nepal), and Marathas (in western India). By 1870, divergence was a global cultural phenomenon. Official statistics became its instrument and indication. Like Malthusian ideas—which were used to explain famine and indentured labor migration in India long before census data could measure population pressure on the land¹⁶—Europe’s superior productivity compared to Asia was taken for granted before it became a fact, sometime after 1850.¹⁷ Even then, belief in Western superiority did not need much empirical support, and its explanation still less, because the new idea of divergence simply translated old ideas about cultural difference into the market lexicon of industrial imperialism. By 1870, the supremacy of white Western men had assumed its modern industrial form.¹⁸ Modern theorists explained economic inequalities by reference to essential differences among people who ranked high and low on the economic scale. Difference explained inequality. Poverty marked inferiority and earned subordination. These basic principles of divergence applied equally to race, class, gender, and civilization.¹⁹

Modern knowledge substantiated inequality and made it sensible. Like the making of the English working class, modern inequality began before large-scale industrialization. As British armies fought the French and Dutch in Europe and overseas, British authors like James Mill opened a cultural space for British supremacy. Across the later decades of divergence, Orientalism provided a flexible tool for explaining Asian backwardness that Karl Marx and Max Weber wrote into the foundations of social science.²⁰ Social Darwinism evolved inside High Imperialism and implicated the poor in all societies. Already by 1845, however, English urban poverty and Irish famine mortality had made economic divergence a political issue in the United Kingdom. Debates about the causes of the Irish famine mirror those about Indian famines: one side blamed native tradition, the other British

¹⁵ J. Majeed, “James Mill’s ‘The History of British India’ and Utilitarianism as a Rhetoric of Reform,” *Modern Asian Studies* 24, no. 2 (1990): 209–24; James Mill, *The History of British India* (London, 1817); David Kopf, *The Brahmo Samaj and the Shaping of the Modern Indian Mind* (Princeton, N.J., 1979); and Kopf, *British Orientalism and the Bengal Renaissance: The Dynamics of Indian Modernization, 1773–1835* (Berkeley, Calif., 1968).

¹⁶ Malthusian ideas were popular among administrators in British India by the mid-nineteenth century, and census data indicating population pressure arrived after 1900. I owe this insight to Rahul Nair, whose dissertation in history at the University of Pennsylvania concerns population discourse in modern India.

¹⁷ Frank, *ReOrient*, argues for this date for Asia as a whole. Dates would differ for comparisons between different parts of Europe and Asia.

¹⁸ Michael Adas, *Machines as the Measure of Men: Science, Technology, and Ideologies of Western Dominance* (Ithaca, N.Y., 1989).

¹⁹ *Cultures of United States Imperialism*, Amy Kaplan and Donald E. Pease, eds. (Durham, N.C., 1993).

²⁰ *Orientalism and the Postcolonial Predicament: Perspectives on South Asia*, Carol A. Breckenridge and Peter van der Veer, eds. (Philadelphia, 1993); Bryan S. Turner, *Max Weber: From History to Modernity* (London, 1992).

imperialism.²¹ Such flatly two-sided debates became standard dramas of modern culture among educated middle classes, who learned to appreciate and normalize poverty as a feature of the modern world.²²

Among middle-class reformers and incipient development experts, it became common knowledge that ignorance, iniquity, and inequity thrived among poor people. These components of poverty became targets for reform. Missionary zeal arrived in India during wars with Napoleon, but British abolitionism launched the age of modern reformism, when free labor became the hallmark of progress just at the time when industry demanded armies of workers.²³ After 1833, identical missionary and social reform efforts spread across the American South, Africa, and Asia. Reformers addressed a standard set of issues. The natives' abuse of women became a favorite theme.²⁴ Development discourse emerged on frontiers of poverty produced by modern inequality, as reformers attacked rigid native hierarchy, despotic native rulers, local corruption, irrational superstition, primitive technology, clannish and tribal closed-mindedness, and blind adherence to archaic ways.

At the same time, however, native tradition—invented, rediscovered, romanticized, valorized, and enforced by law—became national culture.²⁵ In 1880, Weber argued that each nation's unique identity made the national state the rational basis for modern political organization and economic policy-making.²⁶ Weber spent the rest of his life sorting out cultural traits that constituted (universal) modernity and (national) tradition.²⁷ By 1920, modernity and tradition evoked codified collections of cultural practices that respectively promoted and restrained economic progress. National leaders promoted development with modern institutions and rallied the nation with tradition. In South Asia, linkages between modernity and tradition lay

²¹ Joel Mokyr, *Why Ireland Starved: A Quantitative and Analytical History of the Irish Economy, 1800–1850* (London, 1983), 278–95.

²² Two examples of popular poverty appreciation: George Lambert's fund-raising book for his relief mission, *India: Horror-Stricken Empire (Containing a Full Account of the Famine, Plague, and Earthquake of 1896–7)* (Elkhart, Ind., 1898); and Alexander Loveday's 1913 prize-winning essay at Peterhouse, Cambridge, which declared that "Poverty in England, or America, or Germany is a question of the distribution of wealth [. . . while in] India, it is a question of production." *The History and Economics of Indian Famines* (1913; rpt. edn., Delhi, 1985).

²³ Gyan Prakash, *Bonded Histories: Genealogies of Labor Servitude in Colonial India* (Cambridge, 1990); Madhavi Kale, *Fragments of Empire: Capital, Slavery, and Indian Indentured Labor Migration to the British Caribbean* (Philadelphia, 1998).

²⁴ Lata Mani, "Contentious Traditions: The Debate on Sati in Colonial India," *Cultural Critique* (1987): 119–56; and Mani, "The Female Subject, the Colonial Gaze: Reading Eyewitness Accounts of Widow Burning," in *Interrogating Modernity: Culture and Colonialism in India*, Tejaswini Niranjana, P. Sudhir, and V. Dhreshwar, eds. (Calcutta, 1993), 273–90; Chandra Mohanty, "Under Western Eyes: Feminist Scholarship and Colonial Discourses," in *Third World Women and the Politics of Feminism*, Mohanty, Ann Russo, and Lourdes Torres, eds. (Bloomington, Ind., 1991), rpt. in *The Women, Gender, and Development Reader*, Nalini Visvanathan, Lynn Duggan, Laurie Nisonoff, and Nan Wiegersma, eds. (London, 1997), 79–86.

²⁵ *The Invention of Tradition*, Eric Hobsbawm and Terence Ranger, eds. (Cambridge, 1982); Benedict Anderson, *Imagined Communities* (London, 1983); Nicholas B. Dirks, *Castes of Mind: Colonialism and the Making of Modern India* (Princeton, N.J., 2001).

²⁶ Max Weber, "The Nation-State and Economic Policy (Frieberg Address)," *Economy and Society* 9, no. 4 (1980): 428–49.

²⁷ Max Weber, *The Protestant Ethic and the Spirit of Capitalism*, Talcott Parsons, trans. (London, 1930); Weber, *The Religion of India: The Sociology of Hinduism and Buddhism*, Hans H. Gerth and Don Martindale, trans. and eds. (New York, 1958); *The Religion of China: Confucianism and Taoism*, Hans H. Gerth, trans. and ed. (New York, 1964).

deep in “village society,” where traditional order met rural development.²⁸ Combining (Western) modernity and (Asian) tradition became the mantra of national governments as the subordination of tradition to modernity materialized in economic disparities between elites and subalterns. Tradition and modernity became political allies and adversaries as they attached variously to social conflicts fostered by modern inequality.²⁹

The great divergence is therefore neither a singular process nor a simple empirical fact, and it pervades modern history as it combines and connects differently constructed inequalities in and across national borders. In efforts to historicize modern inequality, two approaches are most prominent. One considers territories such as national states to be separate entities. Comparing and aggregating data from separate territories produces larger empirical patterns. The U.S. Census Bureau, United Nations, and all international development agencies use this approach. Its historical and political implication is that each geographical container for data is responsible for conditions inside its borders. Another approach considers territories contextually inside wider networks of interaction, connectivity, and causation. This approach is more common in studies of poor countries subject to foreign intervention and external influence. Dependency theory³⁰ and world-systems theory³¹ are two formal methods of contextual analysis that came out of research on Latin America and Africa, respectively. Studies of colonialism are always at least implicitly comparative.³² Comparative history is a different exercise in light of each approach: independent comparisons consider territories separately, and contextual comparisons consider systems of regional interrelations.

Pomeranz and Wong invite us to explore the overlapping of these two

²⁸ David Ludden, *An Agrarian History of South Asia* (*The New Cambridge History of India*, Vol. 4, Part 4 [Cambridge, 1999]).

²⁹ Richard G. Fox, “Urban Class and Communal Consciousness in Colonial Punjab: The Genesis of India’s Immediate Regime,” *Modern Asian Studies* 18, no. 3 (1984): 459–89; and Fox, “Communalism and Modernity,” in *Contesting the Nation: Religion, Community, and the Politics of Democracy in India*, David Ludden, ed. (Philadelphia, 1996), 235–50; David A. Washbrook, “Caste, Class and Dominance in Modern Tamil Nadu,” in *Dominance and State Power in Modern India: Decline of a Social Order*, Francine R. Frankel and M. S. A. Rao, eds., 2 vols. (Delhi, 1989–90), 1: 204–64.

³⁰ Fernando H. Cardoso and Enzo Faletto, *Dependency and Development in Latin America*, Marjory M. Urquidí, trans. (Berkeley, Calif., 1979); *The Political Economy of Underdevelopment: Dependence in Senegal*, Rita Cruise O’Brien, ed. (Beverly Hills, Calif., 1979); Ernest Feder, *Strawberry Imperialism: An Enquiry into the Mechanisms of Dependency in Mexican Agriculture* (The Hague, 1977); Tony Smith, “The Underdevelopment of Development Literature: The Case of Dependency Theory,” *World Politics* 31, no. 2 (1979): 247–88; Francine R. Frankel, “Modernization and Dependency Theories: Is a Social Science of Development Possible?” in *Development, Politics, and Social Theory: Essays in Honour of Professor S. P. Varma*, Iqbal Narain, ed. (New Delhi, 1989).

³¹ Immanuel Wallerstein, *Historical Capitalism* (London, 1983), provides an overview. On the first Asian phase, see Immanuel Wallerstein, *The Modern World-System*, Vol. 3: *The Second Era of Great Expansion of the Capitalist World-Economy, 1730–1840s* (San Diego, Calif., 1989); *South Asia and World Capitalism*, Sugata Bose, ed. (Delhi, 1990), 21–26.

³² Hamza Alavi, “India and the Colonial Mode of Production,” *Economic and Political Weekly*, nos. 33–35, Special Number (1975): 1235–62; Homi K. Bhabha, “Of Mimicry and Man: The Ambivalence of Colonial Discourse,” *October* 28 (Spring 1984): 125–33; Partha Chatterjee, *The Nation and Its Fragments: Colonial and Postcolonial Histories* (Princeton, N.J., 1993); G. Balandier, “The Colonial Situation: A Theoretical Approach,” in *Social Change: The Colonial Situation*, Immanuel Wallerstein, ed. (New York, 1966), 34–61; Dale Tomich, “The Dialectic of Colonialism and Culture: The Origins of the Negritude of Aimé Césaire,” *Review* 2, no. 3 (1979): 351–85; *Colonialism and Culture*, Nicholas B. Dirks, ed. (Ann Arbor, Mich., 1992).

approaches and to follow evidence in historical sources back and forth between them. It is notable that economic historians of China who write in English typically adopt a national perspective on pre-modern China. This in part reflects the actual closure and self-containment of the huge Chinese economy in pre-modern times; it also reflects imperial and national ideas in Chinese policy, ideology, and statistical data. Historians of Japan also often stress Japan's uniqueness and closure in early modern times. In Japan and China, however, official ideologies and foreign accounts appear to have exaggerated the extent to which their economies excluded the outside world. As Wong indicates, they both seem to have been very much part of an Asian trading world in early modern times.³³ Pomeranz uses an array of data in cross-regional comparisons that lead him out of a strictly national perspective into wider early modern contexts of economic development.

Economic history in South Asia has had a strong contextual impulse from the 1870s, when Dadabhai Naoroji argued that the British economy was enriching itself at India's expense. Naoroji wrote in England. He became a leader of the Indian National Congress. He invented economic nationalism "from below" in the midst of the great divergence, in a country supremely open to world markets under British control.³⁴ It is intriguing to imagine what impact Irish debates might have had on his thinking. The idea spread quickly that the world economy includes exploitative powers that benefit rich countries and deprive poor countries of assets they need to develop. In 1930, Jawaharlal Nehru expressed a homegrown underdevelopment theory by saying, "the great poverty and misery of the Indian People are due, not only to foreign exploitation in India but also to the economic structure of society, which the alien rulers support so that their exploitation may continue." In this light, he clarified the project of nationalism by saying, "In order to remove this poverty and misery and to ameliorate the condition of the masses, it is essential to make revolutionary changes in the present economic and social structure of society and to remove the gross inequalities."³⁵ Contextual dynamics of inequality inside South Asia have remained prominent since Nehru's day. Current academic efforts to track the local impact of globalization have a rich pedigree.³⁶ New connections between the world economy and South Asian localities emerge every day. As I write, globalization seems to have increased violence against women over claims to dowry payments in India.³⁷ In the eighteenth century, multicultural enclaves of commercial capitalism in India seem to have sustained British trades and military adventures throughout Asia.³⁸

Recapturing early modern history in its own context became more difficult as

³³ Lee, "Trade and Economy in Pre-industrial East Asia."

³⁴ Bipan Chandra, *The Rise and Growth of Economic Nationalism in India: Economic Policies of Indian National Leadership, 1880–1905* (New Delhi, 1966); Romesh Chandra Dutt, *The Economic History of India* (Calcutta, 1901).

³⁵ A. Moin Zaidi, ed., *A Tryst with Destiny: A Study of Economic Policy Resolutions of the Indian National Congress Passed during the Last 100 Years* (New Delhi, 1985), 54.

³⁶ For example, Dina Mahnaz Siddiqi, "Miracle Worker or Womanmachine? Tracking Transnational Realities in Bangladeshi Factories," *Economic and Political Weekly* 35, nos. 21–22, Special Issue on Labour and Political Economy (May 27, 2000).

³⁷ Dina Siddiqi, personal communication with the author.

³⁸ See Lakshmi Subramanian, *Indigenous Capital and Imperial Expansion: Bombay, Surat, and the West Coast* (Delhi, 1996); Blair B. Kling and M. N. Pearson, *The Age of Partnership: Europeans in Asia before Domination* (Honolulu, 1979); Michael N. Pearson, *Before Colonialism: Theories on Asian-*

divergence progressed. British imperialists tossed aside Indian partners, and racial divisions hardened in India as British industrial supremacy expanded worldwide.³⁹ Once set in motion, global divergence and modern inequality changed their composition, organization, and conceptualization during four broad periods: 1820–1880, 1880–1920, 1920–1980, and post-1980. Scholars in each period recomposed early modern history accordingly.

It now appears that early modern authors themselves did not live in a world of divergence. Even when they wrote about economic development, they did not describe *diverging* economies or domestic *trends* in inequality.⁴⁰ Before 1800, inequality appears to be a static fact; and among places, it appears to comprise different amounts and types of wealth in different environments. In 1793, for example, Robert Bruce surveyed British activity in Asia without saying Britain was more productive or prosperous. He rather stressed that Britain and France sought to secure *economic* benefits in Asia with *military* victories, and that Britain had used its military “to revive its arts, diffuse its manufactured productions, restore its revenue, and once more, to give splendour to its empire.”⁴¹ Thomas Munro spent thirty years in India working for the East India Company, in its peak years of military expansion, and he concluded that Indians were as much a nation of shopkeepers as the British.⁴² In 1818, James Mill published his triumphal history of British India without claiming British economic supremacy.⁴³ Three decades later, however, Marx took that supremacy for granted.⁴⁴ Beguiled by the factory, Marx and his peers lost sight of early modern facts depicted simply and unabashedly by Bruce and Munro: the British military secured East India Company profits in India. A British alliance between the military and merchant capital had set the new empire in motion. Bruce and Munro did not know how much of that merchant capital was Indian and could never have known how important the India empire would be for British industry.

The surge in English factory production that beguiled Marx apparently began after Waterloo.⁴⁵ British war expenditure supplemented private industrial invest-

European Relations, 1500–1750 (Delhi, 1988); *Merchants, Markets and the State in Early Modern India*, Sanjay Subrahmanyam, ed. (Delhi, 1990); Frank, *ReOrient*.

³⁹ Blair B. Kling, *Partner in Empire: Dwarkanath Tagore and the Age of Enterprise in Eastern India* (Berkeley, Calif., 1976); Thomas R. Metcalf, *Ideologies of the Raj* (*The New Cambridge History of India*, Vol. 3, Part 4 [Cambridge, 1994]).

⁴⁰ William J. Barber, *British Economic Thought and India 1600–1858: A Study in the History of Development Economics* (Oxford, 1975).

⁴¹ Robert Bruce, *Historical View of Plans for the Government of British India and Regulation of Trade to the East Indies and Outlines of a Plan of Foreign Government, of Commercial Economy, and of Domestic Administration, for the Asiatic Interests of Great Britain* (London, 1793), 14.

⁴² For his data and reasons for this conclusion, see David Ludden, “The Formation of Modern Agrarian Economies in South India,” in *The History of Indian Science, Philosophy, and Culture*, Vol. 6: *The Economic History of India, 18th to 20th Centuries*, Binay Bhushan Chaudhuri, ed. (Delhi, 2002). On Munro and his observations, see Burton Stein, *Thomas Munro: The Origins of the Colonial State and His Vision of Empire* (Delhi, 1989).

⁴³ Mill, *History of British India*.

⁴⁴ *Karl Marx on Colonialism and Modernization*, Shlomo Avineri, ed. (Garden City, N.Y., 1969).

⁴⁵ Angus Maddison, “A Comparison of Levels of GDP Per Capita in Developed and Developing Countries, 1700–1980,” *Journal of Economic History* 43, nos. 27–41 (1983).

ments, and Indian revenues supported British war finance.⁴⁶ After the war, industry took center stage. British trade policies to support English industry impoverished weavers in Ireland and India at the same time⁴⁷ as the great divergence began.⁴⁸ After 1880, High Imperialism reorganized global disparities on a larger scale than ever before and began to institutionalize economic development at the same time.⁴⁹ In the 1920s, nationalist movements in Asia and Africa shifted debates about modern inequality into a national framework of analysis under the umbrella of the League of Nations and the British Commonwealth.⁵⁰ After 1950, High Nationalism covered the world of conflict over socialist and capitalist development strategies.⁵¹ Two international meetings represent the official reorganization of modern inequality under High Imperialism and High Nationalism. In the 1880s, treaties in Berlin partitioned Africa and wrote the collective power of rich countries into international law. In 1954, leaders of non-aligned nations met in Bandung, where they defined the First, Second, and Third World, thus formalizing a division of the world into rich and poor, and socialist and capitalist, countries.⁵²

After 1980, modern inequality lost its Cold War guise, High Nationalism declined, the Second World disappeared, and socialist development options gave way to globalization. Moishe Postone describes the new context as characterized by “the weakening and partial dissolution of . . . national state bureaucracies, industrial labour unions, and physically centralized, state dependent capitalist firms.” He explains, “Those institutions have been undermined in two directions: by the emergence of a new plurality of social groupings, organizations, movements, parties, regions, and subcultures on the one hand and by a process of globalization and concentration of capital on a new, very abstract level that is far removed from immediate experience and is apparently outside the effective control of the state machinery on the other.”⁵³ In this new environment, religiously inspired cultural

⁴⁶ Javier Cuenca Esteban, “The British Balance of Payments, 1772–1820: India Transfers and War Finance,” *Economic History Review* 54 (2001): 58–87.

⁴⁷ *Cloth and Commerce: Textiles in Colonial India*, Tirthankar Roy, ed. (Delhi, 1999); Tirthankar Roy, *Traditional Industry in the Economy of Colonial India* (Cambridge, 1999); Mokyr, *Why Ireland Starved*.

⁴⁸ In the nineteenth century, pre-British India appeared to be a long, stagnant past without hope for economic development. Representations by James Mill and Karl Marx are exemplary. For a short account of trends in writing pre-modern Indian history, see David Ludden, “India before Colonialism: The International Impact of Indian Research since 1947,” in *India’s Worlds and U.S. Scholars, 1947–1997*, Joseph W. Elder, Ainslee T. Embree, and Edward C. Dimock, Jr., eds. (New Delhi, 1998), 265–82.

⁴⁹ David Ludden, “India’s Development Regime,” in Dirks, *Colonialism and Culture*, 247–87.

⁵⁰ The epitome of pre-modern Indian history as composed under High Imperialism is the *Cambridge History of India* (Cambridge, 1930). Jawaharlal Nehru summarized the national version of pre-modern history in his *Discovery of India* (New York, 1946).

⁵¹ On the intellectual polarization of economic history around conflicting visions of development, see David Ludden, “Agricultural Production and Indian History,” in Ludden, ed., *Agricultural Production and Indian History* (Delhi, 1994), 1–35. For primary documents, see *Modern Asian Studies* 19, no. 3 (1985), especially Irfan Habib, “Studying a Colonial Economy—Without Perceiving Colonialism,” 355–82, and Dharma Kumar, “The Dangers of Manichaeism,” 383–86. The work under debate is *The Cambridge Economic History of India*, Vol. 2, Dharma Kumar, ed.

⁵² The classic histories of early modern world history in its Cold War guise are William H. McNeill, *The Rise of the West: A History of the Human Community* (Chicago, 1963); and L. S. Stavrianos, *Global Rift: The Third World Comes of Age* (New York, 1981).

⁵³ Moishe Postone, “Political Theory and Historical Analysis,” in *Habermas and the Public Sphere*, Craig Calhoun, ed. (Cambridge, Mass., 1992), 175–76.

nationalism expressed a new vision of modern inequality in and across national borders. Fundamentalism and majoritarianism flourished in rich and poor countries alike amidst invisible mysteries of the market, and in the United States, the great divergence exchanged its Cold War costume to become “clash of civilizations”⁵⁴ and “global anarchy”⁵⁵ infested with threats of global terrorism.

Freed from fears of revolution after 1990, experts have focused new attention on economic inequality untainted by class analysis.⁵⁶ The results are stunning. In an essay written for the World Bank and aptly entitled, “Divergence, Big Time,” Lant Pritchett calculated that ratios of per-capita income between rich and poor countries increased more than six-fold between 1870 and 1985, as income levels dispersed over an ever-widening range of variation and rich and poor economies clustered on either end of a broader spectrum.⁵⁷ Today, inequality is increasing. The gap between the richest and poorest countries is growing. The poorest of the poor are an ever-larger proportion of the world population. Absolute poverty increased in the 1990s most dramatically in Africa,⁵⁸ where an average household now consumes 20 percent less than twenty-five years ago.⁵⁹

These trends implicate history in various ways. Historians who write about high-income countries study a shrinking global elite minority. The population of the rich OECD countries shrank as a proportion of the world population from 20 percent in 1960 to 15 percent in 1993, and may be 10 percent by 2025.⁶⁰ Already, a mere 10 percent of the world’s people live in twelve countries with over \$20,000 per-capita GDP, mostly in the United States (45 percent), Japan (21 percent), Germany (14 percent), and France (10 percent). Rich islands of national history are getting richer and smaller at the same time, which encourages scholars in rich countries to expand their global reach through area studies, world history, cultural studies, and global studies.⁶¹ The best facilities for studying the world sit in rich countries. The United States has a dozen libraries with more books from South Asia than any library in South Asia. Most funding to study the world is in rich countries, where the most prestigious, well-funded academic paradigms emerge for economic and policy analysis as well as for history.⁶² Meanwhile, 80 percent of the world’s

⁵⁴ Samuel P. Huntington, “The Clash of Civilizations?” *Foreign Affairs* 72 (1993): 22–49; and *The Clash of Civilizations and the Remaking of World Order* (New York, 1996).

⁵⁵ Robert D. Kaplan, “The Coming Anarchy: How Scarcity, Crime, Overpopulation, Tribalism, and Disease Are Rapidly Destroying the Social Fabric of Our Planet,” *Atlantic Monthly* 273 (February 1994): 44–76.

⁵⁶ For the latest, see <http://www.worldbank.org/poverty/inequal/>, which is the World Bank’s official inequality web page. An array of current sources of data is available through links on my homepage: <http://www.sas.upenn.edu/~dludden>.

⁵⁷ Lant Pritchett, *Divergence, Big Time* (Washington D.C., 1995), Policy Research Working Paper 1522, Background Paper for the *World Development Report*, 1995.

⁵⁸ On Africa and the recent “rise of the fourth world,” see Manuel Castells, *The Information Age: Economy, Society, and Culture*, Vol. 3: *End of Millennium* (Oxford, 1998), 70–165.

⁵⁹ United Nations Development Programme, *Human Development Report 1998* (New York, 1998). Also available at www.undp.org/hdro/general/past.htm.

⁶⁰ See the Organisation for Economic Co-operation and Development at www.oecd.org. Pritchett, *Divergence*.

⁶¹ David Ludden, “Area Studies in the Age of Globalization,” *Frontiers: The Interdisciplinary Journal of Study Abroad* (Winter 2000): 1–22.

⁶² Rich-country preferences directly determine paradigms for poor-country research at most of the world’s best-funded research centers. Academic tastes among rich-country readers may also influence poor-country research more subtly. See David Ludden, “A Brief History of Subalternity,” in Ludden,

people live and work in thirty-six African and nineteen Asian countries with under \$1,000 per-capita GDP, large proportions in China (34 percent), India (26 percent), Indonesia (5 percent), Pakistan (4 percent), Bangladesh (3 percent), Nigeria (3 percent), Vietnam (2 percent), and the Philippines (3 percent).⁶³ Research and library budgets decline faster than national income as we move down the scale of national wealth. Even getting basic books and journals containing fundamental research on one's own country is impossible for most history teachers and scholars in poor countries. All this material is in New York, Amsterdam, Paris, and London.

Rich people are increasing their share of the wealth. In the thirty years after 1962, the richest 20 percent of the world population increased its share from 70 percent to 85 percent.⁶⁴ Between 1977 and 1999, the richest 20 percent of U.S. households increased their share of national income from 44 percent to 50 percent, and the highest 1 percent increased their share *six times* more, from 7 to 13 percent.⁶⁵ Economic disparities globally, nationally, and locally—in India, China, Europe, and North America—appear to be increasing in comparable, connected ways. In this context, Ken Pomeranz and Bin Wong invite historians to reconsider the rise of modern inequality in light of new perspectives on early modernity fostered by contemporary globalization.

ed., *Reading Subaltern Studies: Critical History, Contested Meaning, and the Globalization of South Asia* (Delhi, 2002), 1–42.

⁶³ These calculations are from 1995 UNICEF data on the UNICEF web site, www.unicef.org. They have since reorganized their site. Readers can find this material on the web, at www.history.upenn.edu/hist388/Hist388imageframeset.html, under my course “Hunger and Poverty in Market Economics,” Week 4.

⁶⁴ United Nations Development Programme, *Human Development Report 1992* (Baltimore, 1992).

⁶⁵ Congressional Budget Office data, analyzed by the Center on Budget and Policy Priorities, reported by David Cay Johnston, “Gap between Rich and Poor Found Substantially Wider,” *New York Times*, September 5, 1999.

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Review Essay
News, Public, Nation

MICHAEL SCHUDSON

THERE IS A PALPABLE NERVOUSNESS when journalists talk about the future of journalism these days. Books are published in the ether, stories break on-line, young people who don't know the difference between an editorial and a news story expertly surf the Web. Is this the end of the several hundred year run of journalism in human history? It seems a moment ripe for assessing the role of the press—and for purposes of this review, particularly the newspaper press—in modern history.

Newspapers have sung their own praises from the beginning, as pillars of democracy, scourges of tyranny, mass communication bards, and builders of community, but few historians or political and social theorists have reflected at length on news as an institution. Nor, with rare exceptions, have literary scholars reflected on news as a cultural form. Media specialists may pronounce journalism to be “the sense-making practice of modernity” and “the most important textual system in the world,” but most humanities scholars have paid no heed.¹

In the corpus of grand social theory, journalism received attention only in passing from Karl Marx, who for some time supported himself as a journalist. Max Weber outlined, but never undertook, a full-scale study of the press. Alexis de Tocqueville wrote a few stunning pages on the American press, still worth close reading, but this pales in comparison to the hundreds of pages he devoted to government and law.

Journalism simply never became a major topic of study among historians or social scientists. Of the famous Chicago School of sociology, only Robert Park, himself an ex-journalist, wrote at length about the press (*The Immigrant Press and Its Control*, 1922) and among leading political thinkers of the past century, only Walter Lippmann, also a journalist, considered the press with intellectual rigor (*Public Opinion*, 1922). Journalism schools have not to this day nurtured a substantial research tradition; near the bottom of the academic pecking order at most institutions (Missouri and Northwestern are notable exceptions), their university colleagues do not expect or demand much from them. This is not to deny that occasional works of substance and influence have come from them, but most work from the journalism schools and too much of the work from the communication research programs that burgeoned after World War II has been undefiled by

¹ John Hartley, *Popular Reality: Journalism, Modernity, Popular Culture* (London, 1996), 32–33.

serious study of leading thought in economics, sociology, political science, or history.

Most people readily (perhaps too readily) grant that the media are important, but identifying their role in human affairs, especially a specific causal role, is difficult. The mass media, through symbols, make possible social coordination, yet this is so general, pervasive, and widely distributed a function that it is hard to trace its specific imprint on human affairs. The mass media are probably more potent in creating solidarities than in breaking them, but building solidarity through culture rather than, say, conquest, is a slow, accretive process all but impossible to measure. Moreover, while the press establishes and sometimes helps expand the borders of consciousness for individuals, it much more rarely determines what people focus their lives on or what moves them to action. The press may surround the attentive news audience with cues, counsel, and guidance to behavior, but it has little say in determining which of these multiple and conflicting tools for living the news consumer will choose to adopt. As communication scholars say, the news may do more to influence what people think about than what they think, and the press may exercise more clout in what it omits than in what it disseminates.²

Most of the time, news coverage moves few to reflection and fewer still to action. It then becomes hard to attribute very much causal weight to the headlines on the rare occasions when they do anger or inspire or mobilize. Even then, how is the analyst to distinguish the effect of the information conveyed from the effect of the legitimation that information gains from being conveyed by a reputable and widely distributed source or from the effect of the way the medium frames the information it offers? Without sorting out these features of news, it is difficult to say whether the effects of news are “media” effects, wrought by the framing or cultural assumptions journalists or their employers bring to news, or “source” effects, engineered by the sources who provide journalists their information.³ News today is only occasionally political in a direct sense, that is, something authored with the purpose of changing policy or power, but it is often the medium for politics. It is not a social trend, but it can amplify social trends; it is not a social movement, but it can contribute to a social movement’s growth or its containment; it is not a moral panic, but it can spread the seeds.

It will always be difficult clearly to identify the power of the press.⁴ Nevertheless, media history in recent years has been taking on greater prominence. In some measure, this is simply because the rapid and radical transformations of the delivery of news in recent years impresses itself upon us. This ranges from the growth of television news in the 1960s to the growing commercial competition to public broadcasting in Europe to the availability of cable and satellite-beamed news delivery, all contributing to the widespread sense that everyday life is increasingly

² The origin of this phrase appears to be in Bernard C. Cohen, *The Press and Foreign Policy* (Princeton, N.J., 1963), 13: “This is to say, then, that the press is significantly more than a purveyor of information and opinion. It may not be successful much of the time in telling people what to think, but it is stunningly successful in telling its readers what to think *about*.”

³ I develop this analysis further in *The Sociology of News* (forthcoming).

⁴ A particularly deft, and skeptical, effort to think through the independent influence of the mass media on political affairs is Jeffrey N. Wasserstrom, “Mass Media and Mass Actions in Urban China, 1919–1989,” in Jeremy D. Popkin, ed., *Media and Revolution: Comparative Perspectives* (Lexington, Ky., 1995), 189–219.

oriented toward information, personalities, and styles originating in or propelled by the mass media. To some degree, growing interest in media history is a happy by-product of the developing “history of the book” subfield. In the United States, Robert Darnton’s stimulating explorations have been especially influential, culminating in his call, in his 1999 AHA presidential address, for more study of “how societies made sense of events and transmitted information about them.”⁵ A “cultural turn” in historical studies generally has also helped, although amidst a vast array of objects from the past suddenly turned read-able, from cockfights to funeral processions, the humble newspaper has not proved particularly fashionable. Most of all, new interest in media history derives from two broad perspectives that place the media in a position of importance, those provided by Jürgen Habermas and Benedict Anderson.

With the work of Habermas, the emergence of the newspaper as a going concern in the eighteenth century takes on world-historical grandeur as a central institution of “the public sphere” and modern democratic discourse.⁶ Before Habermas, there were of course substantial works on the freedom of the press and works on the role of “the fourth estate” as a factor in political life, but not within a broad framework of social theory. This is what Habermas provided.

As is well known, Habermas found that a “bourgeois public sphere” opened up in parts of Western Europe toward the end of the eighteenth century, a social space and set of attendant norms that enabled and encouraged private persons to come together without the sponsorship or surveillance of government to discuss public issues and to do so in a way that placed a premium on the use of individual reason. Newspapers, pamphlets, coffeehouses, and salons were among the key instruments that made this public sphere possible. Unfortunately, this moment of freedom began to be shut down by the mid to late nineteenth century, as the power of capital turned a realm of liberty into an occasion for profit, an opportunity for enlightenment into a marketplace of sensation. Capitalism “refeudalized” public life, even as capitalism had been a primary force in shattering feudalism in the first place.

The particulars of this argument have now been challenged again and again, and few scholars would today accept the Habermasian view as Habermas originally proposed it—and this includes in at least some respects Habermas himself. Habermas presented his work originally in something of an exceptionalist vein—as Weber asked why rational capitalism arose in the West and not elsewhere, Habermas inquires why a bourgeois order, and not others, gave rise to a free domain of reasoned public discourse on politics. While the historical argument has been debated, the category of “public sphere” has been lifted out of it as a normative model of exemplary civic life, and this has inspired research in a wide variety of nations and periods.

The other theoretical account of the media’s role in modern times to attract wide interest among scholars across the disciplines is Benedict Anderson’s *Imagined Communities: Reflections on the Origin and Spread of Nationalism* (1983). In this

⁵ Robert Darnton, “An Early Information Society: News and the Media in Eighteenth-Century Paris,” *AHR* 105 (February 2000): 1.

⁶ Jürgen Habermas, *The Structural Transformation of the Public Sphere: An Inquiry into a Category of Bourgeois Society*, Thomas Burger, trans. (1962; Cambridge, Mass., 1989).

fertile and provocative essay, which covers 500 years of world history in less than 150 pages, Anderson proposes that all nations and, indeed, all communities larger than small villages—and maybe even these—are “imagined communities,” that is, entities that exist in people’s minds as objects of orientation and objects of affiliation. The emergence of national consciousness in the European nation-states was produced by a conjunction of capitalism, print, and the “fatality of human linguistic diversity,” in Anderson’s account.⁷ Anderson gives special credit to newspapers and novels as instruments of national imagined communities, but I think by his own account this works out better for the Americas than for Europe. In Europe, the nineteenth-century spread of national consciousness and nationalist movements was less a product of popular literature than of intellectuals who produced grammars, dictionaries, histories, and high literature in a vernacular language.

With respect to newspapers, Anderson stresses the ceremony of common readership and its role in national consciousness, the idea that everyone is simultaneously reading the same thing, or at least aware of it. This is a wonderful, if exaggerated, image. Circulations of newspapers were small everywhere until late in the nineteenth century. Multiple, often party-oriented, newspapers not only saw the same news from different perspectives but often covered different news events altogether. Still, Anderson has been important and inspiring in drawing attention to the role of print, language, popular literature, publicity, and intellectuals in the rise of national consciousness.

Habermas focuses on the exchange of reasoned opinions about politics in a newly emerging, semi-autonomous public forum in which publicity is both the means of public life and armor against its repression. There is all the difference in the world between a community (Anderson’s focus) and a public (Habermas’s concern); the former tends to imply a common emotional identity, the latter only a common set of norms for public conversation. The former indicates fellow feeling on the basis of interaction (even if it is mediated interaction), the latter suggests civil interaction around a common political subject even in the absence of fellow feeling. Where Anderson examines social membership, Habermas looks to criticism of the state and the formation of liberal institutions. Anderson’s “imagined communities” have nothing to do with liberalism but everything with national consciousness; the Habermasian “public sphere” has everything to do with liberalism, both its achievements and its limitations.

Anderson’s work potentially promotes a much more expansive reading of news than Habermas inspires, a recognition that news is not only the raw material for rational public discourse but also the public construction of particular images of self, community, and nation. It implies that the study of news should be kin to other studies of the literary or artistic products of human imagination more than to studies in democratic theory.

Habermas provides powerful inspiration for many contemporary scholars. Still, the utility of his framework for journalism history is constrained in two respects. First, its emphasis on what is general to bourgeois societies leaves to the sidelines

⁷ Benedict Anderson, *Imagined Communities: Reflections on the Origin and Spread of Nationalism* (London, 1983), 46.

the differences among them—and increasingly media scholars find interest in the differences. Second, his emphasis on the commercialization of the media in the nineteenth century picks up only one part of a two-part development—that journalism as an occupational field professionalized at the same time journalism as a business enterprise commercialized. In the past two centuries, news has come to be a professionally created and commercially distributed product in most parts of the world. The movement from a party or government press to a commercial press accelerated sharply in the 1830s in the United States, the 1850s in Britain, the 1880s and 1890s in Japan. As the press became differentiated from political parties and political movements, it grew not only more driven by commercial considerations but also more organized by self-governing professional norms and practices, although the norms and institutions that conveyed them look different as you move from France to Britain to the United States. Parties retained a firmer hold on newspapers in many parts of Europe than in the United States, while in China, the former Soviet Union, and Eastern Europe, commercialization took off only in the past decade after several generations of state-authorized news. The journalism of France has been notably less oriented to reporting, investigating, and scandal-discovering than Anglo-American media, although in France, Latin America, and elsewhere, a growth of investigative journalism has accompanied a rise in “infotainment” journalism during the past decade.⁸ Newspaper readership, relatively low in the United States and Canada, and declining, has grown in Europe even in the past half-century. Circulation of newspapers per 1,000 population is nearly three times higher in Japan and the Scandinavian countries than in the United States, Canada, France, or Belgium. Britain has a thriving tabloid sector, but the United States, Italy, France, and the Netherlands do not.⁹

With all this variability, global terms like “the public sphere” and “refeudalization” (or “imagined community,” for that matter) are not in the end going to be enough to understand the local terrain of media history. Habermas draws attention, both normatively and empirically, to the central issue of commercialization. What is commercialization and what are its consequences? The profit motive, like, say, sexual desire, can have terrible consequences, but it is one of the forces in life powerful enough to push people ahead in the face of risk, tyranny, and physical and moral hazards. What have been its costs and benefits and its variations across national traditions? The same questions should be asked of the concomitant development of professionalism—the differentiation of journalists as a distinct occupational group with distinctive norms and traditions and, depending on the time and place, some degree of autonomy from parties and publishers. A third central topic is the differentiation of the press from government or state control or, to put it differently, the emergence of legal protections for an independent press. A fourth theme deserves but almost always eludes the attention of media history, for lack of convincing evidence: the changing receptivity of audiences for different

⁸ Silvio Waisbord, *Watchdog Journalism in South America: News, Accountability, and Democracy* (New York, 2000).

⁹ Pippa Norris, *A Virtuous Circle: Political Communications in Postindustrial Societies* (Cambridge, 2000).

types of news, different quantities, styles, topics, and levels of seriousness of reportage.

All these themes are at play in the works considered here (even as the last of them remains in the background). These works are chosen not to offer a comprehensive review of work in media history nor even because they are exemplary but only to give a sense of the landscape. Only the work by Jean Chalaby, *The Invention of Journalism* (1998), offers broad generalizations about journalism as such and has begun to influence others in media history broadly. Chalaby's work is implicitly comparative, as I will indicate; the work by James Huffman is a comprehensive overview of newspaper history in Japan in its first three-quarters of a century and does not aspire to broader claims; Susan Tifft and Alex Jones's study of the *New York Times* examines one newspaper through its entire history and, likewise, makes no overarching propositions. The works together, however, do afford an opportunity to think through aspects of the question of commercialization and professionalism.

THE MOST PROVOCATIVE OF THE BOOKS UNDER REVIEW is Jean Chalaby's *Invention of Journalism*. It is also the most orthodoxly Habermasian in its "rise-and-fall" view of the past 200 years of public discourse. It holds that "journalism" as we have known it over the past century is essentially a product of Anglo-American culture that has diffused, with greater or lesser success, to other nations. As this institution, rooted in the marketplace, developed in mid to late nineteenth-century Britain, it displaced and destroyed a vibrant public life. The extent to which journalism, as it is widely understood today, is a product of a specific Anglo-American "news discourse" of mid-to-late nineteenth-century origins is the claim at the heart of Chalaby's book, and it is worth serious consideration.

There are two parts to Chalaby's thesis. First, he holds that the profession of journalist and a specific discourse or "field of discursive production" called journalism emerged in Britain in the second half of the nineteenth century. Second, he insists that journalism so understood was a retreat from a better model of discourse "political at heart and public in character." Journalistic discourse replaced the "publicity" model of discourse that "prevailed in the press of the pre-journalistic age." As the market principle came to dominate, the press kept the popular classes "in a state of ecstasy" that denied them knowledge of the world and of their place in it.¹⁰

In Chalaby's scheme, publicists, like the writers who produced the unstamped newspapers for a working-class audience in the 1820s and 1830s in Britain, write to articulate the political ideology and interests of a group, especially a social class. They analyze current affairs from "a perspective proper to a social class and thereby develop the aptitude of the members of this social class to see things in their own terms."¹¹ They are normally active members of political organizations.

During the 1830s in Britain, 200 unstamped journals existed at least briefly, half

¹⁰ Jean K. Chalaby, *The Invention of Journalism* (New York, 1998), 1, 2, 5.

¹¹ Chalaby, *Invention of Journalism*, 16.

of them with politics at their center and most of these staking out a working-class, ultra-radical view.¹² These newspapers informed readers of the activities of the political associations they represented and publicized others whose work was allied with them. Chalaby holds that the publicists addressed readers in their political and social dimensions and sought to persuade and struggle with readers' presumed preconceptions.¹³ He then takes this to represent an era of public discourse from which "journalism" represents a decline.

This is an oversimplified view of a remarkable moment in British working-class history. First, the unstamped papers represent a moment rather than a whole era, a year or two surrounding political struggles in 1819 and the half-decade of the flourishing unstamped working-class press of the 1830s. Second, Chalaby's view ignores the half of the unstamped papers that were not principally political. Third, it omits the complexity within the political papers themselves, the fact that these paragons of political virtue—to Chalaby, they were at once principled, dialogic, public-spirited, and pure of heart—were "read and sold less for their political and economic doctrines than for their cheap news." That, at least, is the view of Patricia Hollis, whose 1970 study *The Pauper Press* remains the standard on the subject.¹⁴ Chalaby neglects as well the perturbations in these papers as, in seeking to survive, they moved from radical political essays to police news and efforts to entertain as well as to instruct. Henry Hetherington, one of the leading editors of the radical press, promised his readers in 1834 that henceforth his paper would become "a repository of all the gems and treasures, and fun and frolic and 'news and occurrences' of the week. It shall abound in Police Intelligence, in Murders, Rapes, Suicides, Burnings, Maimings, Theatricals, Races, Pugilism, and all manner of moving 'accidents by flood and field.'"¹⁵

In a word, the journalism Chalaby attributes to a later day and finds a danger to public life was practiced by the publicists of his pre-journalistic Eden. Chalaby seeks to explain how Britain moved from an outpouring of political publicity to the cut-and-dried, conservative, and anti-political "journalistic field." The turning point for him is the repeal of the taxes on knowledge between 1855 and 1861. (In Chalaby's account, this just happens, an uncaused cause.) The tax repeal enabled newspapers to sell on the streets for a penny and created a market and marketplace competition, rather than a "public." In what is a familiar refrain in contemporary writings on mass culture from the left, the public is replaced by the market, the citizen by the consumer. For Chalaby, the new commercial popular press "offers a magic resolution to personal and social problems." It addresses readers' fantasies and constructs "a world of illusions around their readers' dreams." He concludes, "Most differences between public discourse and journalism for the masses originate from the fact that publicists wrote to transform the world, journalists to brighten it."¹⁶

Chalaby claims that after 1855 there was a reduction in political news, although

¹² Chalaby, *Invention of Journalism*, 13.

¹³ Chalaby, *Invention of Journalism*, 29.

¹⁴ Patricia Hollis, *The Pauper Press: A Study in Working-Class Radicalism of the 1830s* (London, 1970), 285.

¹⁵ Hollis, *Pauper Press*, 122.

¹⁶ Chalaby, *Invention of Journalism*, 193.

that is not what his evidence shows. He shows a reduced *percentage* of news about politics among all the news in the papers he samples, but with market incentives making possible a vast increase in the number and size of newspapers, the total quantity of political news must surely have grown. (American data on this point are compelling, even though Gerald Baldasty, the historian who gathered the best data, focused like Chalaby only on the percentage of total news devoted to politics and ignored his own evidence of huge increases in the total amount of political news from the 1830s to the 1890s.¹⁷) For Chalaby, the decline of the parliamentary column and “unmediated” communication between politician and the public is a particularly grievous loss in the commercial press.

Despite Chalaby’s nostalgia and knee-jerk opposition to the marketplace, he still authors a strikingly new formulation in specifying some of the features that define the discourse of journalism. Note especially his simple and important observation that “reporting” is its primary feature—what he calls a “fact-centred discursive practice.”¹⁸ This was a distinctively Anglo-American development. More than Chalaby suggests, the Americans led the way; both British and continental observers of U.S. journalism after the Civil War were appalled at the emphasis on reports rather than essays, commentaries, and editorials. Chalaby offers a useful discussion of “objectivity” as a discursive practice, the “crusadism” of Anglo-American journalism, jingoism (“one way of overcoming the difficulty of producing exciting reading material without offending any substantial group of readers”), and sensationalism.¹⁹ There is more to be done here in describing the history of the discursive forms of journalism—for instance, Chalaby offers just three paragraphs on the emergence of interviewing, an American news invention, but Chalaby makes a genuine contribution in locating media history at the level of genres, texts, and literary forms.²⁰

For all the originality and wit of this book, its thesis of decline and fall runs away with it. Curiously, Chalaby’s sense of a world in a deplorable downward spiral is entirely absent from his earlier article, “Journalism as an Anglo-American Invention.”²¹ The article differs from the book in three respects. First, it is nearly as attentive to American as to British journalism. Second, it gives depth to the concept of a “fact-centred discursive practice” by keeping present a contrast between Anglo-American and French journalism, whereas France is almost invisible in the book. Third, the article offers a multi-faceted historical account of why the Anglo-American press promoted a “fact-centred discursive practice” and the French press failed to do so, an account as much cultural and political as economic. Where the book reduces the explanation of journalistic discourse to market forces, the article suggests that it emerged in the Anglo-American world and not the

¹⁷ Gerald J. Baldasty, *The Commercialization of News in the Nineteenth Century* (Madison, Wis., 1992), 153–57.

¹⁸ Chalaby, *Invention of Journalism*, 5.

¹⁹ Chalaby, *Invention of Journalism*, 147.

²⁰ On the history of the interview, see Michael Schudson, *The Power of News* (Cambridge, Mass., 1995), 72–93.

²¹ *European Journal of Communication* 11 (1996): 303–26. Serious media history seems as or more likely to turn up in communication journals as history journals. Good work has been published in the *European Journal of Communication*, *Media, Culture, and Society*, and the *Journal of Communication*. The journal *Media History* began in 1995.

French for cultural and political reasons, not just economic. He observes that there is a weak hierarchy of discursive forms in the Anglo-American world, where in France the prestige of literary discourse kept the essay, rather than the news report, the dominant literary form. Moreover, there was limited government repression in the Anglo-American cases, compared to the French, and news reporting could develop more freely.

The difference between Chalaby One and Chalaby Two raises an important question: does the profit motive, standing against government censorship and against elite literary castes and forms, produce a better, more free journalism—as the Chalaby article hints—or does it kill off a political press and produce a hopelessly compromised retreat from vibrant political discourse, as the book boldly asserts?

The case of Japan offers a useful perspective on this, though not one that can be made to arbitrate decisively between the two Chalabys. At the time of the Meiji Restoration in 1868, there was essentially no such thing as journalism in Japan. When it began, it borrowed directly from the West, especially the American model. But it took the reporter to be a lowly figure, the essayist a highly esteemed one. Looking primarily to the Anglo-American model, the Japanese, operating from their own traditions, concocted the French!

The Japanese case is well described in James Huffman's *Creating a Public*.²² Huffman has written an exhaustively researched work, lucid, conscious of issues of comparison and theory, even if these enter the careful chronological narrative less often than they might have. Someone who has mastered the complexity and subtlety of social change in the press the way Huffman has is someone whose views on wider controversies of press history would be worth hearing. Huffman's theme in *Creating a Public* is that the public created a press of a certain kind as much as the press brought into being a public. Huffman puts this well: the Japanese press was transformed between 1870 and 1910 from small, gray, dense political papers into newspapers reaching a mass audience, packed with sensation, exposés of corruption, and the sex lives of the powerful, as well as serialized novels and family columns. The newspapers, managed throughout this period by editors and publishers attached to elitist political values and Confucian paternalism, certainly influenced the public, but the influence was in both directions. "The newspapers," Huffman summarizes, "turned the people into citizens; the people turned the papers into mass media."²³ But just how did this happen?

Before 1868, freedom of expression—in domains from puppet theater to book publishing—was tightly controlled to preserve the authority of the shogunate families, government, and the samurai class. Newspapers did not exist, apart from *kawabaran*, occasional one-page black and white broadsheets, until the 1850s. At the time, the Tokugawa government began collecting and publishing foreign news gathered from European newspapers for an audience of officials. The government relied on the foreign press even for news of Japan. The first sustained news publication, begun in 1860, was a government abridgment of a Dutch newspaper published in Java. The first non-governmental newspaper began in 1861, a shipping

²² James L. Huffman, *Creating a Public: People and Press in Meiji Japan* (Honolulu, 1997).

²³ Huffman, *Creating a Public*, 3.

news semi-weekly printed in English by an English grocer for the expatriate merchant community.

At the time of the Meiji Restoration in 1868, then, Japanese journalism had been small-scale, hesitant, short-lived, and more likely to be in English than Japanese. But from March to August 1868, a moment of freedom and confusion before the new rulers had determined policy or consolidated power, a private, political, vigorous, anti-regime press arose. Before the year was out, as the Meiji government consolidated power and influence, Tokugawa-style restrictions on free expression were reinstituted. Only three small non-governmental papers survived—two protected by their publication in Yokohama, a treaty port, and the third decidedly pro-government.

This 1868 journalism of political advocacy is still widely taken to be the prototype for the modern Japanese press, but this may flatter the Japanese press too much. The most distinctive features of modern Japanese journalism date only to the 1870s, when private papers deferential to the government were established with the backing of government bureaus.²⁴ The government directly encouraged a newspaper habit with the establishment of reading rooms and newspaper discussion clubs in towns and villages. Newspapers and nationalism went hand in hand.²⁵ Paternalism characterized both the relationship of the newspapers toward readers and hierarchy in the news organizations—editors at the top, essayists next, and, at the bottom, the newsgatherers, who turned their notes over to office-based writers. The editors of these papers “preferred influence to freedom, a platform to independence.”²⁶ Their papers were political, elitist, pro-government, and willing tools of an establishment intent on civilizing the nation.

Still, the range of topics and assertiveness of criticism grew in the 1870s in this establishment press, the *oshimbun*. In the late 1870s, the *koshimbun*, or “small papers,” also emerged—written in vernacular prose, featuring sensational news, sold cheaply on the streets, short on politics and editorials and long on human interest, with *furigana*, phonetic aids for reading the Chinese characters. At first, the establishment press were untouched by this development; they remained consumed with politics and content with their small, intellectual, and influential readership. But in relatively short order, the two ends of the journalistic continuum would seek middle ground, both moving toward a blend of politics and profit.²⁷

In the late 1870s, the *oshimbun* grew more independent and more confident, jockeying with government censors, especially after a severe new press law in 1875. They even hired surrogate editors whose only job was to serve out jail terms when the newspaper was prosecuted.²⁸ In general, the papers maintained Confucian propriety and loyalty to the national cause, but they also sought to shape policy debates by their own lights and increasingly to champion constitutional government.

Yomiuri, which had been launched in 1874 as the first of Tokyo’s *koshimbun*, in the 1880s began to cover politics and government. A growing politicization of the

²⁴ Huffman, *Creating a Public*, 53.

²⁵ Huffman, *Creating a Public*, 67.

²⁶ Huffman, *Creating a Public*, 63, 61.

²⁷ Huffman, *Creating a Public*, 94.

²⁸ Huffman, *Creating a Public*, 109.

press was met by the government with new forms of political control: an extensive, secret distribution of money to a wide range of journalists and, in a new law of 1882, the establishment of a security deposit system—a fee any paper had to deposit with the government to begin publishing. The deposit system helped eradicate the most poorly financed (and often most critical) newspapers. This was on top of the usual censorship of specific articles, fines, jail sentences, and suspension or closure.

During the 1880s, new leaders of the press were “in every case, less partisan, more news-oriented, and more popular.”²⁹ The largest circulation paper in the country, Osaka’s *Asahi shimbun*, became in the early 1880s independent of party, oriented to news rather than editorial, and dedicated not only to politics but, in the editor’s words, “to record the world’s new expressions, to describe the mysteries of society, to assist in spreading knowledge among the people.”³⁰ *Asahi* entered the Tokyo market in 1888 as a genuine blend of *oshimbun* and *koshimbun*: its pages were gray but its writing simple; it ran serialized novels, it provided *furigana*, and “it marketed its product with the zeal of an American Pulitzer or a British Northcliffe.”³¹ What began to emerge was convergence on a middle way—politics and entertainment, political news rather than political essays, and politics made attractive for a mass audience.

After the promulgation of the Meiji Constitution in 1889, this development only accelerated, along with the general adoption by the press of a strongly nationalist spirit. Even the traditional papers acknowledged that news was a business, that high circulation was a prize dearly sought, and that political influence by itself was an insufficient goal.³² News reporting, not political essays, became the center of political coverage. *Asahi* in Osaka sent out correspondents and established bureaus around the country. *Asahi* and other “middle-road entrepreneurial papers” pioneered the use of the telegraph, the telephone, and much improved printing technology.³³ Like the penny press in the United States half a century before, business-minded, entrepreneurial leaders were the carriers of innovation and, as Chalaby would put it, the sources of a fact-centered news discourse.

There is much more in Huffman’s book, especially concerning the nationalist fervor of the press during coverage of the Sino-Japanese War (1894–1895) and Russo-Japanese War (1903–1905), but I want to dwell for a moment on one thing that is missing from it. As news became the mainstay of the press, reporters’ clubs emerged at parties and cabinet ministries. Huffman is no more admiring of these *kisha* (press) clubs than other observers, but he gives them relatively little attention, as they did not become crucial until the very end of the period he covers.³⁴ Yet they have become one of the most distinctive features of the Japanese news media to this day. They are the subject of another fine book, Laurie Anne Freeman’s *Closing the Shop: Information Cartels and Japan’s Mass Media* (2000). Freeman, a political scientist, writes about the Japanese press of the 1990s from the privileged vantage point of someone who gained access (through *Asahi shimbun*) to the *kisha* clubs in

²⁹ Huffman, *Creating a Public*, 142.

³⁰ Huffman, *Creating a Public*, 144.

³¹ Huffman, *Creating a Public*, 145.

³² Huffman, *Creating a Public*, 178.

³³ Huffman, *Creating a Public*, 174.

³⁴ Huffman, *Creating a Public*, 352–53.

the Liberal Democratic Party headquarters, the Diet, and the prime minister's office. Freeman finds the Japanese press deficient compared to its European and North American counterparts, far too deferential to government and unable properly to police or "audit" the government.

Where Huffman is even-tempered throughout, Freeman is openly critical of the Japanese press, past and present. Where Huffman takes pains to understand the Japanese press in its own terms, Freeman measures the Japanese media against what she takes to be a universal standard of a free, watchdog press. She recognizes that no national press lives up to the ideals she favors, but she convincingly shows that the American and British press have greater access to official sources and greater willingness to speak out on important matters than the Japanese press. Her general historical explanation of this is plausible—that the Japanese press emerged in a country with a limited civil society and a well-established tradition of strict controls on public expression, while the Anglo-American press emerged along with liberal capitalism and the end of formal state censorship.³⁵ As for the *kisha* clubs, she unhesitatingly concludes that they should be disbanded.

A Habermasian model of the fall of the public sphere would not fit Japan. In Japan, there was no golden age, no burst of energies of a press critical of state power, apart from the sputtering freedom of the spring of 1868. The press was fully complicit with power from the beginning, part of an elite modernizing movement. In this situation, commercialism is not the heavy but, perhaps, the hero. Although Huffman finds the commercializing press troublingly nationalistic, he also sees it as indispensable in "creating a public" and bringing the masses into a participatory modern world. Freeman judges the press in Japan deficient not by virtue of its commercialism but by the conjunction of a commercial industry with a state-centered, elite-dominated public culture. And Huffman's achievement is to show how commerce was an agent of an anti-elitism. It did not replace citizen with consumer but, in a sense, initiated the Japanese populace in citizenship through a consumerist appeal.

But here at the heart of Huffman's work, there is an awkward silence. The difference between nation and public, national consciousness and liberal public sphere, is elided. Huffman makes no reference to Habermas; his one footnote to grand theory cites Anderson—and rightly so. The story he tells is about creating a national consciousness, not a bourgeois public sphere. Yet he also aspires to something more, to seeing in the "public" that the newspapers helped create more than a mass of common feeling, as easily turned to nationalism and militarism as to reasoned discourse. I think he means to evoke in the term "public" something more normative—and to be praising Japanese newspapers as agents of its creation. Yet this remains in the end a hint or a hope, no more.

And where is the American experience in relation to this? In *The Trust*, Alex Jones and Susan Tifft take on the most celebrated and distinguished news organization in the United States, the *New York Times*.³⁶ They are not the first to

³⁵ Laurie Anne Freeman, *Closing the Shop: Information Cartels and Japan's Mass Media* (Princeton, N.J., 2000), 171.

³⁶ Susan E. Tifft and Alex S. Jones, *The Trust: The Private and Powerful Family behind the New York Times* (Boston, 1999).

do so, of course. There are two older in-house histories, several substantial memoirs (notably by Harrison Salisbury and Max Frankel), works by David Halberstam and Gay Talese and Edwin Diamond.³⁷ Even so, Jones and Tifft provide fascinating details of family and business history available nowhere else. At first blush, in fact, their book says little about the *New York Times* as a news institution but a lot about it as a family business. The book weaves a collective multi-generational family biography and a business history, a non-fictional *Buddenbrooks*. Its heavy 780 pages are justified only by the great importance of the *New York Times*—otherwise, this account of four generations of the Ochs/Sulzberger family's depressions, dalliances, divorces, and chilly emotional distances between parents and children would seem to have little to commend it to our attention. There are enough characters here for a Russian novel, one in which the scene is the drawing room, the office, and the country estate, not the battlefield, dueling grounds, or the street. The stage is designed more for human comedy than high drama.

Take, for instance, Arthur Ochs "Punch" Sulzberger, who ran the *Times* from 1963 to 1992, deeply reluctant to make tough personnel decisions, backing into them as if he were going to the dentist. One feels for him—after all, he was trying to make his name in the business his father and grandfather had run before him, and he often faced decisions about hiring and firing his cousins! And yet, as comically conflicted as Punch was making decisions *in* the paper, he was rock-solid in deciding *for* the paper. He trusted absolutely in the paper's integrity to battle the government over the Pentagon Papers or, even in his freshman year as publisher, to simply ignore a direct request from President John F. Kennedy to remove reporter David Halberstam from Vietnam. At the age of thirty-seven, the newly installed publisher met at the White House with President Kennedy. Kennedy told him that Halberstam was "too close to the story, too involved" and should be reassigned. Punch did not hesitate for a moment. He told the president that Halberstam had not lost his objectivity and would stay in Saigon. "Punch emerged from the meeting clearly shaken and immediately instructed Halberstam to cancel his scheduled vacation so that it would not appear that the *Times* had capitulated to White House demands."³⁸

Tifft and Jones show the balancing act it has been for the *New York Times* to serve news and profits simultaneously. Huffman also shows a balancing act, but one in which the market plays a generally progressive role, liberating the press from Confucian elitism while promoting a zealous, modernizing chauvinism. Collectively, these works raise as a question what both journalists and historians of journalism have often taken as a settled answer: that money corrupts. This is as much a reflex of professional journalists as it is a theoretical premise in Habermas or Chalaby, and it requires reconsideration on several dimensions. Journalism has emerged and struggled as a marriage of democratic aspirations and commercial ambitions. Its

³⁷ Elmer Davis, *History of the New York Times, 1851–1921* (New York, 1921); Meyer Berger, *The Story of the New York Times, 1851–1951* (New York, 1951); David Halberstam, *The Powers That Be* (New York, 1979); Gay Talese, *The Kingdom and the Power* (New York, 1969); Harrison E. Salisbury, *Without Fear or Favor: The New York Times and Its Times* (New York, 1980); Max Frankel, *The Times of My Life and My Life with the Times* (New York, 1999); and Edwin Diamond, *Behind the Times: Inside the "New" New York Times* (New York, 1994).

³⁸ Tifft and Jones, *The Trust*, 389.

greatest achievement is not engaged citizens or even government officials who know they operate under public scrutiny but the maintenance of an organizational form, built on bottom-line profits, that sustains this over time. The historic achievement is this awkward blend, both Thomas Jefferson and Coca-Cola.

How is it that commercial objectives can aid the public purposes of the press? First, commerce has a populist appeal that breaks through elitist disdain for the masses. This is the Japanese story of commerce versus samurai elitism, and it is also part of the explanation for why the strong commercial and weakly institutionalized literary culture of the Anglo-American tradition produced a more vigorous and more innovative journalism than the French.

Second, commercial imperatives can be combined with others; they need not displace all other motives or means. This is the lesson Tift and Jones implicitly offer. Their theme, convincingly presented, is that the Ochs/Sulzberger family ran the newspaper as a family “trust” rather than a business. “In many ways,” the authors say, “the *Times* operated more like a foundation or an educational institution than a commercial enterprise.” Arthur Hays Sulzberger as publisher (from 1919 to 1961) “deliberately remained ignorant of where the *Times* put its surplus cash, fearful that he might make an editorial decision, however unintentionally, that favored the investment. He kept his personal money exclusively in U.S. Treasury bills.”³⁹ The business culture at the *Times*, such as it was, was “similar to that of a mom-and-pop store.” This began to change when the company became publicly traded in 1969. Arthur’s successor as publisher (after a brief interim command by his son-in-law Orville Dryfoos), his son “Punch,” declared that the newspaper’s first job was to make a profit and then to cover the news. Punch Sulzberger and Abe Rosenthal, editor for much of his tenure, were willing to break down what had been a “rigid compartmentalization” between the news side and the business operations of the paper, “the temple and the countinghouse.”⁴⁰ But with a difference. What was that difference?

Jones and Tift, previously authors of a biography of the Bingham family and its ownership of the Louisville, Kentucky, *Courier-Journal*, contrast these two wealthy newspaper families. The Bingham, they write, did good works because they owned Kentucky’s leading newspaper and, out of the social superiority of their station, embraced an ethic of noblesse oblige. The Sulzbergers, in contrast, believed that “the family belonged to the newspaper” rather than the newspaper to the family, and they felt they had to “prove themselves worthy of the association.”⁴¹ As much as the *Times* reflected assumptions of the privileged classes who owned it, ran it, and wrote it, it also represented their refusal to take the paper as anything but a sacred trust.

Clearly, there is more than one way to be a commercial media organization. There is more than one form of business organization, and there is more than one prevailing ethos that can survive within each form. The laws of the marketplace are variable, even to a degree forgiving, and manifest in practice only as they are interpreted through both national and family tradition.

³⁹ Tift and Jones, *The Trust*, 321.

⁴⁰ Tift and Jones, *The Trust*, 321, 512.

⁴¹ Tift and Jones, *The Trust*, 588.

Tift and Jones do not aspire to social theory; their work is divorced from any conversation with Habermas or Anderson or, for that matter, any other tillers in the field of media history. It is framed by the walls of the organization they describe and the family tree of its owners. This is old-fashioned journalism history, untouched by any of the academy-based intellectual ferment of the past several decades. What their work does it does well, but they leave entirely to others the task of making it bear on broader questions of the media's place in history.

The implication of their work is more dark than what I have drawn from it. They can be read as saying that commercialism *used to be* adaptable to the public interest, so long as it was directed by idiosyncratic families, some of them nobly committed to the general good. Now that more and more media outlets are run by fewer and fewer corporations, and these corporations increasingly are dominated by Wall Street investors, refeudalization has at last taken hold. When family capitalism finally expires, the corporate capitalism that remains will not be tolerant of the mixed motives of bottom line and public service.

This is a common view among many conscientious observers of the present scene. I cannot be sure it is wrong, but I think it is premature. No one has yet worked out why fact-centered discursive practices, centered in commercial institutions, should have had as long as a run as they have of routinely criticizing, embarrassing, and exposing government leaders in the United States and elsewhere, and even occasionally criticizing, embarrassing, and exposing powerful corporations. No one has yet probed what protects professionals in media organizations, so anxious have media watchers been to raise the alarm about what endangers them. Nor do we know just how far to take the metaphor that discourse speaks us rather than we it—that, in this case, the language of observation, analysis, and public accountability that characterizes the news may have set roots deep in our cultural soil and so have some hold over the media giants even as the giants try to take hold of it.

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Reviews of Books

METHODS/THEORY

PEKKA MASONEN. *The Negroland Revisited: Discovery and Invention of the Sudanese Middle Ages*. (Humaniora, number 309.) Helsinki: The Finnish Academy of Science and Letters. 2000. Pp. 599.

The medieval “Negroland” of the title refers to a portion of West Africa lying south of the Sahara desert, west of Lake Chad, and north of the equatorial forest zone that adjoins parts of the Atlantic coast. This perhaps unexpected and certainly arbitrary province of the human experience came to constitute a fascicle of European historiography through a lengthy process of intellectual accumulation and imaginative fabrication. The author offers the work reviewed here as a case study in the “archaeology of knowledge . . . an attempt to reconstruct the way in which European knowledge of African history has evolved by pursuing its textual genealogy through the previous historical and geographical literature” (p. 30). The chronological scope of the study “covers a long period reaching from the Middle Ages, when European scholars, traders, and cartographers heard the earliest rumors concerning the fabulous Land of the Blacks, up to the first decades of the twentieth century” (p. 26). Looming prominently on this intellectual landscape were the famous medieval West African realms of Ghana, Mali, and Songhai; in their shadow, gradually accumulating knowledge also spread out a complex array of numerous lesser places, polities, and communities, many imperfectly understood today. “Personally,” Pekka Masonen explains, “I dislike the current tendency to lay stress in academic writing on ‘brevity’ and ‘readability’ at the expense of scholarship—especially when readability is a pretext for careless documentation and discourse” (pp. 57–58). While perfectly readable, this book is unusually generous in conception, ample in scope, and meticulously documented. The author intends “that this volume would consist of a miniature library, providing useful material for African students of history, which they could use in their own works” (p. 57). Many readers may, in fact, find this volume most useful as an encyclopedic reference work.

Chapters two through five trace sequentially and in exquisite detail the origin, interpretation, and dissemination of the diverse European and Arabic sources of

primary evidence about the interior of medieval West Africa. It would be fair to generalize that information accumulated very slowly (sometimes in odd places) and that, until the nineteenth century, it remained insufficient to allow even the beginning of responsible historical discourse. One unfortunate legacy of this age, perhaps borrowed originally from the worldview of the medieval Arabic writers, has been a scholarly propensity to conflate evidence separated by centuries, as if nothing of significance in West Africa ever changed. Early modern European writers about West Africa knew a fair amount concerning some coastal communities and next to nothing about the interior; this reviewer would have preferred to see those few latter speculative scraps of information about the unseen situated intellectually within the context of what the writers actually thought they knew, rather than being introduced in isolation.

Over the course of the nineteenth century, the discovery of important new Arabic sources, along with insights gathered by European explorers, allowed William Desborough Cooley and Heinrich Barth to frame two versions of a first rough draft for the medieval history of the West African interior (chapter six). Masonen skillfully explores the possibilities and limitations of these drafts (along with those of some competitors) and correctly recognizes Barth as the superior interpreter, not least because he chose to draw upon Arabic works produced in the region itself. During the generation of the “Scramble for Africa” that flourished between 1880 and 1920, French colonial afflatus came to dominate the regional historiography (chapter seven). The author warms in confronting “Babylomaniacs” who not only indulged in orgies of racist diffusionism, spurious etymology, and folkloric confabulation but would even cite Gustave Flaubert’s *Salammbô* as a primary source for the ancient history of Ghana (p. 478). Masonen notes correctly that West African oral historical tradition first entered European historiography via this early, largely French, colonial genre. However, he misses an opportunity to spotlight a cascade of painful ironies with current resonances. Authentic Afrocentric orality of the generation that fathered *négritude* first entered Western discourse not as a critique of logocentric Cartesian rationality but as a perfectly logical explanation and

justification for the foreign dominance of Africans, while in some quarters postmodern de[con]struction has even removed the barriers of factuality that once separated the conceptual universe of *Salammbô* from that of Cooley and Barth.

Among its other virtues, this book may stimulate a number of valuable new historiographical departures. While the author's primary focus lies in the past, his comments on present realities are also telling. He rightly draws critical attention to the theoretically and methodologically primitive state of western Sudanic historiography, its apparently impermeable intellectual insularity, its tendency toward "source fetishism" (p. 13) in the Orientalist tradition, its excessive reliance upon blind appeal to dubious authority both written and oral, and its neglect of important archaeological evidence.

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MAGHAN KEITA. *Race and the Writing of History: Riddling the Sphinx*. (Race and American Culture.) New York: Oxford University Press. 2000. Pp. vii, 214. \$45.00.

Maghan Keita ably scrutinizes several interwoven topics, most conspicuously, and in his own idiom, "black historiography," the "Afrocentric model of historical writing," and "the epistemology of blackness" (p. 52). The word "Afrocentric" has been traced by Derrick Alridge to W. E. B. Du Bois, who employed it in the early 1960s. During the 1970s, Molefi Kete Asante appropriated the term, insisting that he was the only person equipped to define it, and asserting that even the holy archangels Du Bois and Cheikh Anta Diop had an imperfect and immature grasp of a concept that finds ultimate, unfalsifiable expression in his own pontifications. Subsequently, it became a catchall "floating signifier," nebulous, unstable, and infinitely mutable.

Keita's book appeared in the same year and from the same publisher as Mia Bay's excellent *The White Image in the Black Mind: African-American Ideas about White People, 1830-1925*, which employs numerous well-known primary and secondary sources that were equally available to, but unmentioned, by Keita. Astonishingly he slights St. Clair Drake's two-volume *Black Folk Here and There: An Essay in History and Anthropology* (1987), arguably the most authoritative treatment of the black "vindicationist" tradition with its gentle mockery of Afrocentric enthusiasms and Egyptocentric extravagances. He refers to opinion pieces by K. Anthony Appiah, Mary Lefkowitz, and George Will, but Stephen Howe's sneeringly brilliant *Afrocentrism: Mythical Past and Imagined Homes* (1998), with its condescending but erudite survey of the historiography, is ignored.

Keita correctly asserts that the construction of modern racism is a product of Enlightenment science as it developed in the eighteenth century. One recalls

Daniel J. Boorstin's brief treatment of the topic in *The Lost World of Thomas Jefferson* (1948) and Thomas Gossett's in *Race: The History of an Idea in America* (1963). Indeed "scientific" racism was so pervasive by the 1780s that J. G. Herder, J. F. Blumenbach, and Anders Sparrman found it necessary to challenge at least some of its assumptions, while G. L. Buffon, Jefferson, and Joseph Priestley contributed to others. Black readers in later years welcomed translations of Count Volney's *Les Ruines, ou Meditation sur les révolutions des empires* (1791) and Abbé Grégoire's *De la Littérature des Negres* (1809). Such works bolstered the vindicationism of Edward Wilmot Blyden and the writings of the once-enslaved autodidact, John E. Bruce, in Marcus Garvey's newspaper, *Negro World*. Bruce remarkably eludes Keita's attention, as does William H. Ferris, a member of the American Negro Academy and of Garvey's inner circle, whose *The African Abroad* (1913) I have treated elsewhere. Also neglected is Drusilla Dunjee Houston's readily available and widely discussed *Wonderful Ethiopians* (1926).

Although its title promises more than it delivers, this work has strengths that will make it necessary reading for students of African American history. The chapter on W. L. Hansbury is an insightful treatment of an inexcusably neglected scholar. The subtly ironic essay on Frank Snowden lays bare the contradictions in the admittedly distinguished work of its subject. Keita's book demonstrates hard work, common sense, and a great deal of original brilliance.

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DAVID CANNADINE. *Ornamentalism: How the British Saw Their Empire*. New York: Oxford University Press. 2001. Pp. xxiv, 263. \$25.00.

Once the purview of established university chair holders for whom empire radiated outward from center to periphery, British imperial history has undergone some important transformations in the last two decades. Not only have all manner of practitioners—literary theorists, cultural critics, and feminists—rushed in, but former colonials of all varieties have also explored terrains that others feared to tread, especially where the impact of empire on the metropole is concerned. The new generation of scholars has brought with it new tools for research as well, from Foucauldian analysis to poststructuralism to critical race theory, all in an effort to understand the role of British imperialism on both a local and a global scale and to recast the fixity of those structural identities in the process. Surveying these proceedings, David Cannadine has lighted upon the paradigms offered by Edward Said's *Orientalism* (1978) as the most influential of all recent developments. He does so because of its insistence on the role of "otherness"; because of the emphasis he believes it places on race as a category of analysis; and because of the attention he suggests it pays to the experiences of colonial natives. Canna-

dine's new book is his rebuttal to the biases he perceives in recent scholarship on imperialism. In this eye-catching, synthetic brief for imperial history from the perspective of imperial Britons, Cannadine attempts to reclaim empire from contemporary Saidian influences and to make a case for it as a "class act": the province of social history rather than the unwitting hostage of racial (and, to a lesser degree, gendered) interests, whether historical or contemporary.

Does it matter that Said is a straw man in this account, or that he can hardly be characterized as someone who championed subaltern "victims" as the true subjects of colonial history? Evidently not. Cannadine's narrative leads readers through a simulacrum of the grand imperial tour. Colonies, mandates, and monarchs dominate, with geographical realities like "India" only briefly interrupting the march to the end ("Dissolution") of empire. Relying on a vast secondary historiography, Cannadine reiterates his point about how class trumps race by offering examples across time and space. These are often illuminated by photographs that struggle to fulfill the interpretive promise of "ornamentalism." From the twin images of the Maharaja of Gwalior and Lord Curzon (1899) that frame the opening to the one of Queen Elizabeth and King Hussein (1984) that closes the book, the pictures produce a decidedly orientalist effect, especially in the absence of any sustained visual analysis. By the end, Cannadine seems almost diffident about his overarching thesis. How else to explain the appendix, which attests in full and apparently unapologetic postcolonial mode to his own imbrication in a postwar decolonizing culture? Readers may wonder why ornamentalism merits a book rather than an essay, but only if they underestimate the Anglo-American appetite for narratives of empire that eschew the kind of attention to racial violence and struggle characteristic of some of the best work in the new imperial history.

Whether the events of September 11, 2001, will dull those appetites remains to be seen. More pressing for historians is to recognize the political significance of Cannadine's rhetorical choices. Unlike many of his contemporaries contributing to the recent *Oxford History of the British Empire* (1998–1999), Cannadine has not opted for dyspepsia as his register for responding to what many see as the depredations wrought by recent comers to imperial history. Rather than pathologizing Said and his followers (who are in any event a diverse and by no means slavish lot when it comes to the power and possibilities of Orientalism), Cannadine acknowledges them and even anoints a few of their innovations in a series of ornamentalizing gestures reminiscent of that signature export of high imperial culture: patronizing indulgence. Critics of postcolonialism will no doubt rejoice at the caricature of *Orientalism* that Cannadine stages here. But if to ornamentalize is to trivialize, then this book has a lot to account for, since, as historians of modern Britain know, to dismiss or even subordinate race to class is to risk turning a blind eye to its ongoing impact in

Britain, from London to Oldham and beyond. Those interested in doing imperial history in the twenty-first century are well advised to read Cannadine's book against its real "other." This is not Said's *Orientalism* but rather Lord Bhikhu Parekh *et al.*, *The Future of Multi-Ethnic Britain* (2000), which argues, among other things, for an end to the term "English" because of its racially coded connotations and the often murderous hatred of communities of color it has bred in the postimperial British Isles. The task of ornamentalizing that is an unenviable one indeed.

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J. G. A. Pocock. *Barbarism and Religion*. Volume 1, *The Enlightenments of Edward Gibbon, 1737–1764*. New York: Cambridge University Press. 1999. Pp. xv, 339. \$49.95.

J. G. A. Pocock. *Barbarism and Religion*. Volume 2, *Narratives of Civil Government*. New York: Cambridge University Press. 1999. Pp. xiv, 422. \$49.95.

Edward Gibbon's *The History of the Decline and Fall of the Roman Empire* (1776–1788) is probably the most famous and perhaps the most misunderstood history written in the past three centuries. It is often cited, and is even well known in popular culture (Bugs Bunny glances at a parody of it on a vampire's shelf in an old Warner Bros. cartoon), yet more than two-thirds of it, concerning the Greek Eastern Empire, has been largely ignored. Its memorable title has also become falsely associated with popular accounts of the moral decay of the early, Julio-Claudian empire described by Tacitus and Suetonius—a period that Gibbon, who began with the "Five Good Emperors" of the second-century Antonine dynasty, did not even address. These misconceptions aside, the multivolume work has the stature of a monument to Enlightenment culture (though not to philosophe agendas). Study of it has enjoyed a resurgence in recent years, including David Womersley's recent critical edition of the entire text and Patricia B. Craddock's authoritative two-volume biography of Gibbon. How much there is left to say on Gibbon himself, or his masterpiece, is a question one might be excused for asking.

The answer, however, would be "a great deal," and if one has any doubts about that, they are dispelled after reading the first two massive volumes of J. G. A. Pocock's ongoing study. These get us only to the eve of Gibbon's writing the first volume, which appeared in 1776 (a year otherwise notable in intellectual history for the death of David Hume and the publication of the *Wealth of Nations* by Adam Smith—two North Britons who feature prominently in the second of these volumes). Pocock's study is not, as one might expect, another biography—though biographical details both enter into his account and prove significant—but an attempt, largely persuasive, to situate Gibbon in as

many different intellectual and political contexts as can be imagined or reasonably described from the evidence, and through that exercise (which is far from complete at the end of volume two) to arrive at a much richer understanding of the *Decline and Fall*.

There is an impressively symphonic quality to Pocock's major works, at least in their design. In *The Machiavellian Moment* (1975), his study of the transmission and transformation of ancient ideas of republican virtue through medieval, Renaissance, seventeenth-century English, and North Atlantic filters, he began with a dazzling analysis of a millennium of intellectual history up to Niccolò Machiavelli, slowed the pace notably for a middle section on Machiavelli's contemporaries, and picked it up again for a concluding sprint through the seventeenth and eighteenth centuries. There is something of the same here, and what we have is the first two movements—each with several melodic lines simultaneously developed. Volume one of *Barbarism and Religion* does not cover a vast chronological sweep, but it does follow Gibbon's life (and, importantly, the lives of his father and grandfather) and travels against the backdrop of a very complex and distinctively English political and religious climate. The goal of this is more obvious than the road toward it: to demonstrate how and under what circumstances the former apostate, turned militiaman, turned gentleman of letters arrived at his understanding of late antiquity, and of the proper method and suitable style for recounting that understanding to the reading public. There was clearly a much longer and more complex ferment than one can glean from a surface reading of his *Memoirs* alone; the inspirational moment in 1764 when he mused "amidst the ruins" of the Capitol may explain the occasion of his writing the *Decline and Fall*, but not the process that permitted that occasion to occur, much less its results. Along the way, we get a wonderful account of the often conflicting streams of English history from the Elizabethan settlement through the Civil War, the Restoration, the revolutionary settlement, Jacobitism, and the advent of a culture of party, commerce, and civility.

Volume two provides the more relaxed andante to this opening allegro: here, Gibbon himself almost slips from view till the very last, as Pocock presents sequential free-standing studies of the major civil histories that provided the narrative exemplars from which Gibbon could depart (he could not really imitate them since they principally dwelt on more modern times). These include two major continental historians (Pietro Giannone and Voltaire), and his major older British contemporaries (principally the Scots Hume, William Robertson, John Millar, Adam Ferguson, and Smith); there is not much here about English neoclassical history writing, which was in a state of decline, although there is a brief but interesting account of the reception of Hume's major rival, Catharine Sawbridge Macaulay. As with volume one, these studies are valuable in their own right, but they also provide a further context to the writing of the first volume of the

Decline and Fall. (So relentless is Pocock's attention to strict chronology that Robertson's *History of America* [1777] does not feature in his account of that historian, because it appeared too late to have any effect on Gibbon's first volume, and because it belongs to a different intellectual context: that of the American Revolution.) They are, however, only half the story. One of the unresolved questions here is exactly how Gibbon solved the problem of models for his *later* volumes, on the Byzantine east, after his history had taken him in directions, or painted him into corners, that he had probably not anticipated when he first conceived it. One answer would seem to be that Gibbon needed to rely to a much greater extent than has been acknowledged on ecclesiastical historians, and we can expect that there will be a parallel to volume two, later in this series, examining that genre.

Each of these volumes also stands on its own as an exploration of two other intellectual questions not directly related to (or at least not circumscribed by) the study of Gibbon. In *The Enlightenments of Edward Gibbon*, which is dedicated to the memory of Franco Venturi, the question is to what degree England experienced an Enlightenment at all, and how its "rhythm" differed from the continental and Scottish versions. The plural in the title is not an affectation. The case here is very much for multiple enlightenments, that of the philosophes not being the same as that of the *érudits*, and the European being no more singular (or normative) than the English. A secondary theme in this volume is provided by the argument of Arnaldo Momigliano that there were distinctive historiographic streams from Renaissance to Enlightenment—narrative, philosophe, and antiquarian or "erudite"—and that Gibbon was the man who managed to integrate them in the *Decline and Fall*. *Narratives of Civil Government*, dedicated to the memory of Momigliano, takes up this latter theme more directly, though the emphasis is squarely on the narrative (Hume) and the philosophical (Voltaire) streams, with rather less stress on erudition in the mode of Lodovico Antonio Muratori and Sébastien Tillet. For any historiographer for whom Momigliano's famous essays have become central—which is to say most of us working on the seventeenth and eighteenth centuries—this is a welcome addition to the corpus: the least satisfactory part of Momigliano's overall argument was his explanation of exactly *how* Gibbon achieved this synthesis, and by what stages.

These are not easy volumes to read, their pages filled with gallicisms, lengthy extracts, and highly complex sentences weighted with qualifying clauses. The nuances will be lost on anyone unfamiliar with some of the ongoing debates in the history of ideas (and particularly the history of historiography), or with the architecture of the *Decline and Fall* itself. The turns in the argument are conducted with such precision, and at such a pace, that a napping reader may soon be left behind. But in combination, they represent a very significant advance in our understanding of Gibbon.

and of the intellectual and political worlds in which he lived. We are not there yet: there is no climactic third movement imminent, and at the current rate one might expect several more volumes before we hear it.

One of Pocock's first principles of intellectual history has been that authors end up saying both more and less than they intend, and that what they intended and what we take them to mean depends not only on their own, but also on our, context. "Barbarism and religion," Gibbon's most quotable phrase, had one meaning in his time and another in ours. Indeed, it resonates differently between the appearance of these 1999 volumes and the post-September 11, 2001 world in which we must now read them.

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JOHN FARRENKOPF. *Prophet of Decline: Spengler on World History and Politics*. (Political Traditions in Foreign Policy Series.) Baton Rouge: Louisiana State University Press. 2001. Pp. xvii, 304. Cloth \$65.00, paper \$24.95.

All the major works on Oswald Spengler—H. Stuart Hughes, *Oswald Spengler: A Critical Estimate* (1952); Anton Mirko Kocktanek, *Oswald Spengler in seiner Zeit* (1968); and now John Farrenkopf's book—see him as a prophet of our times. But the times have changed. Hughes and Kocktanek placed Spengler in the intellectual and artistic currents of the turn to the twentieth century, the turn away from the Enlightenment; Farrenkopf, writing at the turn to the twenty-first century, views him from the perspective of the decade after the end of the Cold War, which had briefly raised the hopes that the world would follow the pattern of Western democracy and free markets. Farrenkopf's book, with its endorsement of Spengler's conclusion that "world history is ultimately irrational, destructive, and uncontrollable" (p. 283), could well have been written after September 11, 2001.

It is questionable whether Spengler, with the perspective of a German neoconservative of the immediate pre- and post-World War I Germany, and with his hatred of the modern world, was able to understand fully the direction in which this world was moving. Farrenkopf credits Spengler with six major achievements in intellectual history. He rightly points out that Spengler "made a seminal contribution to the liberation of modern historical thought from the limitations of its Eurocentricity" (p. 17) in his comparative approach to world history, although his own political philosophy, with its vision of a "Pax Germanica," was not only Euro- but essentially Germanocentric. There is little indication that in his readings he followed discussions outside the German-speaking area. Spengler recognized only two influences on his thought: J. W. von Goethe and Friedrich Nietzsche. Farrenkopf is right in noting that Spengler "though he revolted against fundamental ideas of the German historicist tradition" actually accepted basic assumptions of this

tradition (p. 77), notably its concentration on the state and power politics, but more importantly its methodological intuitionism, which rejected any systematic analysis using clear concepts and saw the writing of history as closely related to poetry. Of the remaining five achievements with which Farrenkopf credits Spengler, it is true that Spengler proceeded comparatively and that he helped to shatter the optimism and idealization of the idea of progress central to the zeitgeist of the nineteenth century. However, he was by no means the only writer to do so; pessimism was already part of the new zeitgeist. Yet the argument that Spengler "helped to blaze the way for innovative social science research on civilization" (p. 18) is difficult to maintain, for not only was Spengler not taken seriously by social scientists, but he rejected the very foundations of rational approaches to the study of societies and civilizations on which the social sciences rested. The new awareness of the non-Western world and the turn to anthropology and semiotics in the second half of the twentieth century came from changing political and social realities of which Spengler, with his colonial attitude, had little understanding.

After these critical remarks, I would like to stress my positive assessment of Farrenkopf's book as the most substantive of the various studies of Spengler. Farrenkopf, more clearly than any of the other writers on Spengler, distinguishes between two stages of his philosophy of history and his politics. Although Spengler insists that he never deviated from his conception of history in the *Decline of the West* (1918–1922) as "a virtually eternal series of independent cultural cycles" incapable of communicating with each other, Farrenkopf argues that, with *Man and Technics* (1931), Spengler accepts a "tragic, catastrophic vision of history as a largely integrated process" (p. 192), this time beginning in prehistoric times and leading to the self-destruction of mankind through the irresistible dynamics of technological progress and ecological destruction. Similarly, Farrenkopf traces the transformation of Spengler's political views from advocacy of "quasi-democratization of Wilhelmine Germany" (p. 2), which has generally been ignored, to his neo-elitist neoconservatism, which, facing stoically the inescapable reality of modernity, rejects both the romantic nostalgia of the past and the profane populism of the Nazis.

Farrenkopf's study is not only a book about Spengler but also a formulation of his own belief that the destruction of the modern world, including the democratic legacy of the Enlightenment, is unavoidable. Yet the world of 2002 with all its horrors is too different from that of Spengler to permit elevating him to the status of a prophet for our time. The humane values of modernity are threatened, but they deserve to be defended.

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SUSAN WELLS. *Out of the Dead House: Nineteenth-Century Women Physicians and the Writing of Medicine*. Madison: University of Wisconsin Press. 2001. Pp. xii, 312. Cloth \$57.95, paper \$22.95.

Susan Wells uses the methods of literary criticism to decode historical documents, providing a rich analysis of writings by women physicians throughout the nineteenth century. That was the "golden age" of women in medicine, before the reforms advocated by the Flexner Report restricted the field almost exclusively to men. Wells notes the temptation to argue that women developed an alternative form of medical practice. Wisely, she makes a more limited claim: that female physicians made many of the same therapeutic choices as men but that their experiences and the responses they met were very different. Drawing on Judith Butler's notion of gender as performance, Wells demonstrates how women physicians constructed their identities within the various rhetorical strategies available to them.

The first substantive chapter examines the extent to which men and women conducted medical interviews differently. Women physicians took special pride in their ability to listen empathetically to their female patients and elicit their "heart histories." The intimacy that developed enabled female physicians to intervene actively in the lives of their patients, offering them advice on morality, financial matters, and religion as well as on the care of their bodies.

The following chapter analyzes the extensive writings of Ann Preston, who graduated from the Women's Medical College of Pennsylvania in 1850 and remained there first as professor of physiology and then as dean. By describing herself as acting in ways that were indistinguishable from those of male physicians, Preston advanced the interests of the college but hurt herself both personally and professionally.

In the next chapter, Wells first compares mid-century student theses at the Women's Medical College with those at the School of Medicine of the University of Pennsylvania, an institution attended only by men. Although the two groups of theses were similar in most respects, women students were far more likely to criticize the medical profession and to employ satire. The chapter concludes with a fascinating analysis of a group of theses by African-American women students at the Women's Medical College in 1870, demonstrating how they negotiated issues of race as well as gender.

In the two succeeding chapters, Wells analyzes the contrasting rhetorical styles of Hannah Longshore, who aligned herself with "irregular" medical practice, including homeopathy, the water cure, mesmerism, and spiritual healing, and Mary Putnam Jacobi, a prominent physician who fully endorsed the new scientific medicine emerging in the late nineteenth century. While Longshore used travesty to convey her ambivalence toward the medical profession, Jacobi frequently adopted the cool, dispassionate tone of the

male medical writer. The final chapter explains how women physicians succeeded in gaining entry to both public dissections and clinical lectures, thus upsetting notions of female delicacy and raising the fearsome possibility that men as well as women could be the object of the medical gaze.

Although Wells offers subtle analyses of a wide variety of writings, she occasionally could have delved even more deeply into some of the texts she examines. She argues, for example, that Preston based her 1863 lecture, "Nursing the Sick and the Training of Nurses," on her recent, long bout of illness. It is unclear, however, exactly how Preston's personal experience of suffering informed her argument that patients resembled children.

Wells carefully avoids most literary jargon, although some historians may find her writing too dense. But they should persevere. By challenging historians to embrace new ways to examine texts from the past, this book makes a valuable contribution not only to the literature on women in medicine but also to historical studies generally.

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CAROL GRONEMAN. *Nymphomania: A History*. New York: W. W. Norton. 2000. Pp. xxiii, 238. \$24.95.

Carol Groneman's history of nymphomania addresses a topic that has long needed a full-scale analysis by a feminist historian and raises nearly all of the important questions about this ill-defined and now obsolete disease paradigm. Her book elucidates two main points. The first is that "As new sexual rules develop in the twenty-first century, anxieties persist: How much is too much? How much is not enough? Is there a healthy, normal, natural amount of sex? And who decides?" (p. xvii). The second point is that women, far more often than men, are determined by their partners and/or physicians to be outside the "normal" boundaries, either as "frigid" or as sexually insatiable, or, paradoxically, both at once. In the case of women whose desires were thought to be excessive, "nymphomania was in the eye of the beholder" (p. 70). Groneman depicts the search by physicians and psychologists since the nineteenth century for a sexual norm for women as a kind of unrewarding Goldilocks quest: this one is too hot, this one is too cold, but where is the woman who is "just right"? She also points out how rarely the male counterpart, satyriasis, was diagnosed compared to the legions of women labeled nymphomaniacs since the 1850s.

Some of her critique of the medicalization of women's sexuality sounds familiar, but this is hardly Groneman's fault. Like the symptoms of "hysteria," assertive sexual behavior by women that caused trouble for their male partners was diagnosed as a disease and treated by physicians, usually without raising any questions about the legitimacy of enforcing a norm that was not

and probably could not be determined. The mere existence of such a disease paradigm was reassuring to those deeply invested in the androcentric model of sexuality: it validated the male adolescent fantasy of women with unappeasable sexual appetites, while condemning as pathology any demands on adult men beyond those they were willing and able to satisfy. "Normal" female desires were satisfiable by vaginal orgasm in coitus on the husband's schedule. Needless to say, this model was a good fit to a minority of actual women. Men apparently liked to believe in the existence of nymphomaniacs as long as they did not have to have sex with one. Moreover, as Groneman points out, historically men have suspected women who were not satisfied with heterosexual intercourse of other kinds of trouble, including career ambitions and political activism (p. 58).

While there are numerous references in this book to the relationship between the concept of supposed nymphomaniac insatiability and the relative ineffectiveness of coitus as a means of producing orgasm in women, there is no extended discussion of its significance to the development of the disease paradigm, which might have been illuminating. Groneman does give us a wonderful quote from Margaret Sanger about the "average man's sexual technique," which the famous sexual radical says is like "an orangoutang trying to play a violin" (p. 40), and she discusses the contributions of both Alfred C. Kinsey and William H. Masters and Virginia Johnson to the public debate on vaginal orgasm (pp. 89–91, 123–24). Groneman is definitely in her element in the chapter on "Nymphomania in the Courts" (pp. 95–120), and in her later development of this topic as a modern legal and moral theme (pp. 174–79). The "diagnosis" of nymphomania was used from the turn of the nineteenth century until quite recently to undermine the credibility of charges of rape and incest on the grounds that nymphomaniacs typically and pathologically imagined sexual assaults and then made charges on the basis of their fantasies. Where sexual intercourse had clearly occurred, the alleged nymphomaniacs could be charged with inciting and consenting to sexual act, thereby invalidating a charge of rape and exculpating the defendant in incest cases. Even in the notorious *Giles vs. Maryland* case of 1961–1967, in which three young African-American men were accused of raping a sixteen-year-old-girl, the issue of the accuser's "promiscuity" and possible nymphomania affected the Supreme Court's eventual remand of the case to the Maryland Court of Appeals for retrial and the subsequent clearing of all three defendants.

Groneman's book would have benefited from more depth of historical documentation prior to the nineteenth century, and particularly from a discussion of nymphomania's predecessor disease paradigms, among them "greensickness," hysteria, and so-called "widow's and nun's melancholia." There are also a few annoying minor errors, such as the reference to Richard von Krafft-Ebing's 1886 work, *Psychopathia Sexua-*

lis (p. 29), as *Psychologia Sexualis*. Taken as a whole, however, this book is a valuable addition to the historiography of women's sexuality.

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MARK C. CARNES, editor. *Novel History: Historians and Novelists Confront America's Past (and Each Other)*. New York: Simon and Schuster. 2001. Pp. 351. \$26.00.

This book follows *Past Imperfect: History according to the Movies* (1995), a volume in which sixty scholars and writers contributed essays about films that purport to represent the past. Mark C. Carnes conceived of that book as a way to explore the hotly contested boundary between fact and fiction, history and art. But he found that the differences in the values and vision of those who work with words and those who work with images resulted in a "fundamental disjunction of purposes." A historian might have a good reason to watch movies with a critical eye, but the exercise did not necessarily shed new light on the nature of history or art. So Carnes turned to the history in novels, asking twenty historians to write about twenty novels, from Gore Vidal's *Burr* (1973) to Tim O'Brien's *In the Lake of the Woods* (1994). Then he asked each of the (living) novelists to respond—hence the last three words in the book's subtitle.

Readers who pick up the book hoping for combat will be disappointed. The historians are full of admiration, even awe. It is not simply that the novelists write so well; it is also that they know their subjects, characters, and scenes from the inside out. They have "the freedom," as John Mack Faragher says of Jane Smiley, "to imagine a past that is in some ways truer than history" (p. 147). Numerous historians identify instances in which novelists used that freedom to imagine facts, forces, and contexts years before historians managed to document them.

The novelists are never less than respectful. They know that they live in a land where historians—in monographs, textbooks, popular histories, book reviews, op-ed pieces, documentaries, and even radio and television news—are widely considered to be the guardians, if not the proprietors, of the past. If blurbs on the backs of historical novels are any indication, many trespassing novelists take comfort in historians' blessings.

Yet many of the novelists, themselves toilers in the libraries and archives, seem sincerely impressed by how hard historians work and how much they know. "It was clear," Madison Smart Bell writes of Michel-Rolph Trouillot, "that the author knew a hell of a lot more about the Haitian Revolution than I probably ever would" (p. 198).

The volume is not entirely a love fest. James M. McPherson takes Russell Banks to task for numerous factual errors that do not contribute to his story. Banks responds, in one of the few replies that is longer than the original essay, with a lecture on the uses of fact in

fiction. Joanne Freeman argues that Vidal misunderstood eighteenth-century notions of honor.

But no one provokes for provocation's sake. Vidal, who can be (hilariously) ruthless to the "squirrel scholars" who cross him, is exceedingly polite. Edmund Morris is not here. Nor is E. L. Doctorow, who has infuriated historians on numerous occasions, including the time he was asked if Evelyn Nesbit and Emma Goldman ever met, as they do in *Ragtime* (1975). "They've met now," Doctorow replied.

These essays are readable and thoughtful, but there are few surprises. If you know the historian and the novel, you can predict what's coming. That is primarily because the historians are not, by and large, interested in what most interests novelists: the sound of words, imagery, the shape of the story, voice. They approach fiction no differently than they approach history, discussing what the novelist got right and what he or she got wrong, the analytic ends but not the literary means, the content but not the form.

The novelists are less predictable, and in some of their essays (Don DeLillo, Banks, Larry McMurtry, and O'Brien) the vision and passion we treasure in their fiction are on display. But overall they are not as engaging as critics as they are as novelists—as each of them would be the first to admit.

Carnes is obviously aware of the book's limitations. He gives the last word to O'Brien, who in four unforgettable paragraphs expresses his skepticism about talking too much, or too abstractly, about stories. "To extract the 'meaning' from a work of fiction," he writes, "is like extracting hydrogen from a molecule of water. The atomic bonds are broken; fluidity goes; you end up with a fistful of gas" (p. 344).

But even O'Brien admits we can and must talk about stories. Sometimes good talk sheds new light on literature. Sometimes it simply encourages us to read it. This book encouraged me to read one novel I had avoided and two others I had overlooked. For that alone, I am grateful to Carnes and his contributors.

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DOMINICK LACAPRA. *History and Reading: Tocqueville, Foucault, French Studies*. (Green College Lectures, Green College, University of British Columbia.) Buffalo, N.Y.: University of Toronto Press. 2000. Pp. 235. Cloth \$35.00, paper \$19.95.

This collection of four essays by Dominick LaCapra is deceptively slim, for it may in fact be several books in one. On one level, LaCapra's book is a state-of-the-field analysis of both history and literary studies, and French studies in particular. Believing that the long wave of theoretical activity in these fields has finally crested, LaCapra endeavors to reevaluate disciplinary boundaries with the aim of fostering greater communication between history and literary studies—between the "history" and "reading" of the book's title.

On another level, history and reading refer not to academic disciplines but to the act of reading historical texts. In his first essay, a version of which was previously published in the *AHR*, LaCapra offers an interesting taxonomy of historical methodologies. He details five basic approaches to historical texts ranging from an ostensibly objective approach that is unconcerned with the problematic of reading itself (dubbed the "seemingly self-sufficient research paradigm") to a deconstructionist approach that, conversely, focuses on the act of reading to the point that historical context can verge on irrelevance. Between these two extremes are three other historical methodologies that, to a greater or lesser degree, grapple with the complex relationship between the reading subject and the historical text as object. One of these methodologies, the one that LaCapra clearly advocates and that he has explored in his earlier writing, is what he terms the "dialogic" approach.

As the name would suggest, LaCapra's dialogic method owes an important debt to Mikhail Bakhtin. But it seems equally indebted to Sigmund Freud, and although LaCapra does not draw the comparison explicitly, he clearly sees in Bakhtin and Freud a variety of shared concerns. Both recognized the fundamental importance of the past (whether the historical past of society or the past of the individual analysand) for insight into the present; both advocated a form of "dialogic exchange" with the past—a constant series of questions and exploration of various possibilities—as a means of guarding against a unidirectional imposition or "projection" of the analyst's concerns onto the object of analysis; and both recognized that the past was a complex place where hidden meanings and meaningful silences abounded, making analysis an ongoing process rather than an end to be achieved.

LaCapra is particularly interested in the subject of trauma, and much of his recent work—and his study of the Holocaust in particular—has focused on the ways in which trauma resists conventional narration, whether by the historical actors who create texts or by the historians who read them. And it is with respect to trauma that LaCapra sees the dialogic method as particularly important, because in addition to reading texts for what they intend to say (the province of traditional historiography), and for what they reveal without saying explicitly (the focus of practitioners of close textual analysis who search for unexamined preconceptions beneath authorial intent), the dialogic method probes texts for what they cannot say—the traumas that resist articulation and even consciousness. LaCapra argues for an historiography that "would allow trauma to register in the (perhaps never fully successful) attempt to work it through" (p. 62).

Here, in this invocation of an historiography that "works through" trauma, is the crux of LaCapra's dialogic method: the act of historical analysis, for LaCapra, can never be the simple extraction of information from a text because both the text's expression

and the analyst's subjective reading are anything but simple. Only a "conversation" between text and reader can probe the ambiguities of expression while at the same time guarding against the analyst's own projection. And indeed the subjectivity of the analyst is fundamentally important to LaCapra, who rejects the possibility of a truly objective observer and insists that "the subject-position(s) and voice(s) of the historian are an integral component of historiography complicating research" (p. 26). As a means of not privileging his own "subject-position," LaCapra, throughout his book, enters into dialogue not only with historical texts but with other commentators (a conversation often held in the voluminous footnotes), the end result being a kind of analytical free-for-all in which both the intentionality of the text and the objectivity of LaCapra himself are problematized.

The middle chapters of LaCapra's book offer analyses of Alexis de Tocqueville's *Old Regime and the French Revolution* and Michel Foucault's *Folie et déraison: Histoire de la folie à l'âge classique* (1961), the English text of which, *Madness and Civilization* (1965), is an abridged translation. LaCapra's discussion of these texts is meant to function on several different levels: first, he hopes to encourage cross-disciplinary communication, believing that "scholars working in the traditions of one of these important figures rarely read either the works of or commentary on the other" (p. 5). Second, LaCapra hopes to show how both Tocqueville and Foucault, despite their apparent differences, were essentially dialogic in their approaches. Not only were both profoundly interested in a conversation between past and present (as opposed to a purely antiquarian interest in the past), but each of them staked what LaCapra calls "internally dialogized" positions, questioning themselves and assuming a variety of contradictory subject positions. Finally, these texts are meant to be the object of LaCapra's own dialogic analysis.

LaCapra's readings of Tocqueville and Foucault are masterful. Perhaps the highlight of this middle section of the book is his discussion of certain inherent contradictions between the radical implications of Tocqueville's analysis and the conclusions Tocqueville nevertheless drew from it. Specifically, LaCapra argues that Tocqueville was prevented by his "liberal-conservative" political stance from considering even vaguely socialistic practical alternatives to the woeful tale of state centralization, the rending of the social fabric, and the atomization of individuals. One can almost hear LaCapra trying to whisper Durkheimian possibilities into Tocqueville's partially covered ears.

As in his analysis of Tocqueville, LaCapra is candid in his expression of dissatisfaction with Foucault's refusal to suggest practical alternatives to the tragic tale he tells. But here I found LaCapra's argument less convincing. While it may be true that Foucault, like Tocqueville, offered no practical alternatives, this refusal, unlike Tocqueville's, was not a question of turning a blind eye to the consequences of his analysis.

Foucault offered no societal alternatives because, to him, any articulation of a societal category (whether normative or nonnormative) was inherently coercive; whatever possibilities of liberation he might have envisaged were therefore individual, not societal. His alternative to modern sexuality, for example, was not alternative sexualities but rather the nameless pleasures and bodies beyond the categories that language inevitably imposes. Ultimately, Foucault was less interested in attaining civil and political rights for prisoners, gays, and the insane than in problematizing the inherent rationality of the categories that made their existence as "species" possible in the first place.

Readers will inevitably find bits and pieces of LaCapra's bold book with which they disagree. But such differences of opinion are likely to be expressed more in the spirit of joining a fascinating conversation, and therefore testament to the merits of LaCapra's dialogic method rather than a criticism of it. This book is extraordinarily provocative and raises questions that historians will feel compelled to confront. At the very least, the author's discussion of historical methodology will be required reading in many a graduate seminar. It is an important and timely book.

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GIUSEPPE PAPAGNO. *Un modello per la storia: Materiale, attività, funzione*. Foreword by MAURICE AYMARD. Reggio Emilia: Edizioni Diabasis. 2000. Pp. xv, 331. L. 38,000.

The cover's vivid detail from a Benoit Mandelbrot fractal painting sets the tone for this book, which spins models within models and decorates them with colorful, intricate detail. As Maurice Aymard remarks in a perceptive preface, Giuseppe Papagno does not propose his "model for history" as an explanatory device but as a tool for understanding historical processes. Or rather, a set of tools: for Papagno's material-activity-function triad comes in multiple versions depending on the historical problem at hand. All versions, however, consist of asking how some actor (individual or collective) deploys artifacts (*materiale*) in repeated actions (*attività*) with some visible, durable consequences (*funzione*) for social life. For reasons never explained, Papagno dissolves actors into activities, thus obscuring his attributions of agency. He makes much of interactions among the other three elements: materials, activities, and functions. Early in his book, indeed, Papagno makes claims for the three elements as components of self-maintaining systems and even lays out five "laws" of dynamics in such systems; those claims disappear as the book moves into its historical applications.

A specialist in Iberian colonial expansion, Papagno here expands his analyses to Italy and to European history at large. After sixty pages of general reflections, he turns to small applications of his scheme in polished essays averaging about twenty-five pages each, ranging

from Portuguese government in the fourteenth century to the origins of the Italian national flag. Each essay reworks an earlier paper written, and usually published, some time between 1988 and 1999, inserting the material-activity-function scheme somewhere along the way. Topics fall into three groups: important persons including Dom Pedro of Portugal, architect Leon Battista Alberti, conqueror Napoleon Bonaparte, and film writer Cesare Zavattini; Italian historical themes; and "men, nature, and landscape," concentrating on readings of the physical environment as a historical palimpsest. The essays do not cumulate into a general argument or even successive applications of the same method but take up a playfully wide variety of questions, with the earlier conceptual work providing hooks on which to hang observations. In the case of the Portuguese prince Dom Pedro (1392–1449), for example, the three-part scheme helps Papagno show that Dom Pedro's teachings on the art of good government accepted the conventional medieval account of the state's means and functions but substituted good works that would glorify the crown for works that would glorify God. A series of concrete fifteenth-century examples (e.g. the conquest of Ceuta) then illustrate the difficulty of applying any such principle amid the harsh realities of a segmented, expanding empire. Out of the contradictions of Dom Pedro's composite model, argues Papagno, emerged a new (and more coherent) model of the absolutist state.

Papagno's essay on the Italian flag runs from 1797 (when cities within the Repubblica Cispadana fought over how centralized a state their new flag would represent) to 1997 (when the Italian government finally proposed to fly the banner on public buildings continuously instead of on holidays alone). It intertwines two themes: Italian unity or segmentation on one side, and representations of the state on the other. Papagno takes the imposition of the House of Savoy's seal on the red-white-green tricolor from unification to 1946, for example, as representing the uneasy coupling of two legitimating principles, one top-down, and the other bottom-up. Fascists, he notes, did not alter the flag itself but capped it with their own bundled sticks.

In the course of such essays, Papagno repeatedly draws inspiration from the unlikely combination of Lucien Febvre, Fernand Braudel, Karl Popper, and Karl Polanyi. He also provides illustrations and asides from sources as diverse as Edgar Allen Poe, Geographic Information Systems, and *2001: A Space Odyssey*. Since Papagno explicitly disavows explanation and seldom follows any single line of argument more than a couple of pages, no reader should search the volume for an exportable model of inquiry, a serious challenge to previous historiography, or deep insight into major trends of European history. This book succeeds in surprising, diverting, amusing, irritating, and occasionally illuminating. That should suffice to justify its publication.

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DAVID GROSS. *Lost Time: On Remembering and Forgetting in Late Modern Culture*. Amherst: University of Massachusetts Press. 2000. Pp. xii, 199. \$29.95.

David Gross's new book deals with what psychologists call episodic memory: the memory of a past different from the present as embodied not only in personalities and events but also in ideas, values, and emotions. In the history of such memory, he finds a basis for appreciating the many layers of meaning that underpin contemporary culture, and so he accords it an importance that transcends the niche psychologists have assigned it. His broadly conceived interpretation is well focused on this topic, although along the way he offers observations on many of the issues about memory that have recently preoccupied historians: commemoration, the social construction of memory, memory and trauma, memory and historical understanding, and the implications of the electronic revolution for memory.

Gross asks the ingenuous question: is there any value in remembering the past for one's orientation toward life? He works out his erudite reply in light of the way human understanding of memory's value has changed over time. Amid the constant historical interplay between remembering and forgetting, he contends, the long-range trend has been to displace the value of remembering with that of forgetting. In premodern times, memory held a preeminent place in Western culture. From ancient Greek and Roman rhetoricians to medieval clerics, memory was perceived to be the essential ingredient of character, defining one's identity, ethics, and creativity. The modern temper, by contrast, has placed a greater premium on forgetting, a long-term revaluation that has accelerated in the late twentieth century with the advent of a media-driven market economy. In modern times, forgetting has come to be understood as a means of clearing the way for intellectual innovation. The effect has been to sever the ancient association of memory and imagination. Having lost its identity as the inspired manifestation of a transcendent reality, memory has come to be associated with timeworn, stultifying habits of mind. Imagination has been reconceived as an independent mental faculty, the autonomous source of human creativity.

Gross sorts leading thinkers of the Western tradition into rememberers and forgetters. Proceeding systematically, he makes the best case he can for both. He then considers the topic of memory's value in light of the twentieth-century's newfound understanding of the power of social context to shape the frameworks of collective memory. He follows with a panoramic review of the way such contexts have shaped and reshaped the conceptual schemes with which Western culture has embraced its remembrance of the past, from the religious imagery of medieval Christendom, to the political imagery of the emerging nation-state, to the cultural imagery of the media in late modernity. In the process, he addresses with refreshing indepen-

dence of mind a number of issues prominent in recent historiographical discussions about memory, among them the waning appeal of the Platonic notion of a collective unconscious; the reasonableness of forgetting for survivors of atrocities; the adversaries in the *Historikerstreit* as comrades in the defense of remembering; the failure of the theorists of constructed social memory (and historians beholden to them) to discriminate between invented and received tradition; and the insidious power of media (notably television) to limit our intellectual horizons to its factitious conflation of past and present.

Gross closes with a meditation on the future valuation of memory in terms of its pessimists and optimists. Pessimists see the power of a media-driven market economy as overwhelming, portending a deepening historical amnesia. Optimists, while acknowledging media as today's culturally dominant force, hold out hope for memory's redeeming power to counter this present-minded banality. Gross casts his lot with the optimists, affirming his conviction that the dialectic of remembering and forgetting never disappears. In the course of his review of the forces promoting the modern banishment of the past, he showcases some rememberers who, against the intellectual tide of their times, argued persuasively for the creative power of memory, notably Sigmund Freud, Marcel Proust, Henri Bergson, and Walter Benjamin. Although they valued different kinds of memory, they were at one in their appreciation of memory's capacity to make us aware of "non-contemporaneous" realms of the past and so permit us to form a more textured understanding of the human condition. In their hopes, Gross situates his own. More than an apology for a renewed appreciation of the received wisdom of the past, he explains why the past matters to an age that seems to have forgotten the deep sources of its identity. Among the many books on memory and history that have appeared over the past decade, this one stands out as an imposing work of synthesis.

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COMPARATIVE/WORLD

RONALD GRIGOR SUNY and MICHAEL D. KENNEDY, editors. *Intellectuals and the Articulation of the Nation*. Ann Arbor: University of Michigan Press. 1999. Pp. vi, 430. \$49.50.

Thirty years or so into the resurgence of research on the nature of nationalism and national identity, scholars have tended to support one of two schools of thought: one that asserts that nations are real, tangible and historical phenomena that grow out of prenatal ethnic bonds ("perennialist" or "primordialist," depending on the permutation), and another, currently much more popular school that views nations as uniquely modern and constructed, the products of the imagination of states and/or intellectuals who concoct

national identity out of shreds of the past in order to exploit the communal ties and strong emotions that they have invented (this school is often referred to as "modernist" or "constructivist"). The editors of this volume bring together a selection of new and previously published articles that all testify to the importance of intellectuals in the defining of national identity, regardless of the schools to which the authors belong. For Ronald Grigor Suny and Michael D. Kennedy, intellectuals "appear to have the greatest agency in the shaping of national understanding, propagating the values of the nation, disciplining the people internally, and enforcing the rules and boundaries of the constituent people" (p. 2). While acknowledging that both the "perennialist/primordialist" and "constructivist" schools accord intellectuals an important role in nationalism and the nature of a given national identity, they ally themselves with those "who have seen intellectuals, not merely as reflective of what exists, but as constitutive of the nation itself . . . intellectuals here are the creators, not only of nationalisms, but of the more universal discourse of the nation, of the very language and universe of meaning in which nations become possible" (p. 3). Where intellectuals are creators, nations are created; the editors favor the modernist position on the origins of nations. Their contributors, sometimes uncomfortably, follow their lead.

In this volume, only Alexander J. Motyl's "Inventing Invention: The Limits of National Identity Formation," is explicitly theoretical. All, however, have theoretical implications. Janet Hart's "Reading the Radical Subject: Gramsci, Glino, and Paralanguages of the Modern Nation" examines the "political paralanguages" of Antonio Gramsci and Dimitris Glino and their conceptualizations of the revolutionary behavior of intellectuals under fascism. Other contributors examine the activity of intellectuals in several national contexts, for the most part eruditely and informatively. These articles include Yuri Slezkine's on Soviet anthropologist N. Ia. Marr, Andrzej Walicki's on Polish nationalism, Khachig Tololyan's on the place of the poem "Ter Getzo" in Armenian national consciousness, John-Paul Himka's on intellectuals and national identity in Galician Rus', Katherine Verdery's on postsocialist uses of national symbols in Romania, and Kennedy's on the intersections of liberalism and nationalism among businessmen in Poland. Facing the usual difficulties in reviewing a diverse collection of essays, I will focus on just a few contributions.

One of two exceptions to the modernist rule in this volume, Motyl's article is hostile to the constructivist position. After critiquing the formulations of influential modernists Eric Hobsbawm and Benedict Anderson (whose "invented traditions" and "imagined communities," respectively, have exerted a powerful pull on the imaginations and exertions of scholars of nationalism), Motyl argues that "national identity is . . . always and everywhere possible" (p. 70). Motyl's assertion threatens the constructivist insistence on the

modernity of nations and nationalism while expanding the possibility of discovering nations and nationalism in unlikely places. He concedes that nationalism is more common and more likely to arise in the modern era, thanks to the technology of modern communication and the existence of powerful centralized states, but he convincingly insists that "there can be nationalism without nations and nations without nationalism—a proposition that rests on a crucial distinction between identity and ideology that constructivism has failed to appreciate" (p. 71). Motyl's conclusion is that constructivism would be more useful if it abandoned "the language of invention and imagination and the unwarranted theoretical claims they imply" (p. 72).

Walicki ("Intellectual Elites and the Vicissitudes of 'Imagined Nation' in Poland") shares Motyl's dislike of constructivism. For Walicki, the constructivist argument is "one-sided because too many facts contradict the definition of nations as something contingent, merely invented, or imagined" (p. 260). He believes, however, that the constructivist emphasis on the role of intellectuals in "shaping national identities" is salutary (p. 260). Walicki argues that the "'constructionist' approach can be preserved if it ceases to be coupled with risky generalizations, if it is not treated as the only, universally applicable key to all national phenomena" (p. 261). In the body of his article, he outlines phases in the history of Polish national identity, from medieval (ethnolinguistic) to early modern (civic) to modern (historico-political, with multicultural, unitarist, and federal variants). Walicki demonstrates the difficulty of asserting the inventedness of nations without straying from Polish social, political, and historical realities (according to Walicki, the most invented of the Polish national identities failed because they did not resonate with the Poles themselves).

Verdery ("Civil Society or Nation? 'Europe' in the Symbolism of Romania's Postsocialist Politics") examines the various ways that Romania's postsocialist intellectuals and politicians (as in most East European societies, these were often the same people) attempted to define Romania's relationship to "Europe." In her stimulating discussion, Verdery focuses on "a small group of politicians and intellectuals who see themselves as partisans of 'Europe'" (p. 302). Her article underscores the difficulty that southeastern Europe's multiethnic societies face under postcommunist conditions in which nationalism (in different forms) and the concept of "civil society" competed for predominance in political discourse. She describes her work as an analysis of the "political economy of the symbolism around these notions" (p. 303). Ultimately, her article describes a Romania in which a universal language of European integration, human rights, and democracy failed to move any people beyond a limited urban/intellectual stratum; the symbolism of the nation had to be invoked to appeal to a mass audience, which was of course critical at election time.

The articles are faithful to the title of the volume, but they vary stylistically and defy simple categoriza-

tion, since their subject matter and periodizations differ. Walicki's covers the early modern and modern periods, Himka's the nineteenth century, Slezkine's the twentieth century, and Verdery's and Kennedy's the postcommunist period. Walicki and Himka paint with broad brushes, whereas Tololyan, Slezkine, and Kennedy examine narrow case studies. Kennedy's article is the most esoteric, as it analyzes the discourse of business people in postcommunist Poland for articulations of national consciousness. The book, therefore, will probably not be useful to teachers of any course but one on nationalism itself; several of the individual articles, however, will be welcome additions to syllabi for more general/regional courses.

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ELAZAR BARKAN. *Guilt of Nations: Restitution and Negotiating Historical Injustices*. New York: W. W. Norton. 2000. Pp. xli, 414. \$29.95.

How do nations amend historical injustices in which they are implicated? The question is imbued with the urgency of recent newspaper headlines showing the proliferation of claims by various groups charging past injustices at the hands of governmental and business entities. These latest claims for restitution of wages, land, and art objects, and reparations for suffering due to genocide, slavery, and other offenses, are often made with competing versions of history vying for space in the public discourse. The increased frequency of such claims, irrespective of their legitimacy, shows that history has become, more than ever, the site for political contestation in seemingly endless battles between perpetrators and victims. Yet the dimensions of these conflicts mimic eternal duels between good and evil that are part of the human condition and often defy easy characterization and resolution.

The purpose of Elazar Barkan's ambitious attempt to examine the efforts of historically victimized groups in different parts of the world to exact justice from offending nations is, therefore, not simply to lay blame or even to acknowledge suffering but to espouse an international standard of morality and justice that may have wide applicability. Using a comparative approach, Barkan seeks to build a theory of restitution by identifying common approaches and analyzing the ingredients that produce success and failure among the claimants. While his subjective selection of "the most compelling cases" can be debated for what is included (three chapters on claims against the Nazis) and what is left out (Palestinian claims against Israel; the whole African continent), his premise is clear. Barkan believes that success in amending historical injustices requires a well-choreographed negotiation in which both victim and perpetrator can share in the rewriting of history and memory.

This possibility of a shared, negotiated history is a marked departure from the static and dichotomous historiography of a bygone era that allotted space only

for “winners” and “losers.” It reflects a postcolonial sensibility whose beginnings Barkan traces to the end of World War II. It is facilitated by the ultimate demise of realpolitik Cold War ideology and the emergence of international concern for human rights and social justice. Restitution campaigns also embody a rearticulation and expansion of Enlightenment principles, once reserved for individuals, into rights for minority groups, which Barkan dubs a “Neo-Enlightenment” position. Because so much is at stake, the growing agitation by different aggrieved groups to redefine themselves by altering the historical discourse through restitution and reparation claims will only intensify. But as Barkan shows in each of his twelve chapters and the book’s conclusion, the debate that surrounds these claims, although painful and protracted, can play a useful and ultimately healing role in mending a nation’s social fabric. For this to happen, group rights must be accorded some legitimacy, especially if group persecution was its forerunner.

Barkan credits the Marshall Plan, engineered by the United States to rebuild Europe after World War II, as the progenitor of modern restitution movements. Because the plan included Germany, it fostered a non-punitive attitude that allowed the defeated German nation to promote its own self-rehabilitation by its compensation of Jewish victims. This win-win model would prove to be the glue that permitted the crafting of future successful restitution agreements.

Such examples confirm the instructive intent in Barkan’s case studies. The book is divided into two sections: one dealing with cases of restitution in the aftermath of World War II, and the second with the aftermath of colonialism. He uses these as historical markers and as source material while building a trans-historical analysis with copious analogies and lessons. It should be noted that Barkan defines “restitution” as the return of wrongly taken goods, to distinguish it from “reparation,” which he defines as recompense for what cannot be returned. Once defined, however, his actual usage blurs this distinction.

In the book’s first chapter, on the “Faustian Predicament” that made possible German reparation to Jewish victims of the Third Reich, Barkan also provides a negative comparison to the aftermath of slavery in the United States. While Germany emerged from the Holocaust by focusing on denazification and enforcing an official policy in which “Jew-hating disappeared from public life” (p. 16), after the Civil War the official policy of the defeated white South was racism and continued subjugation of blacks. This is one of the striking examples of missed opportunities for national catharsis that Barkan addresses in the last chapter of the book.

Barkan attributes the fact that reparations for slavery is still slightly below the radar (but rising) in American public opinion, despite the long-running campaign by African Americans’ descendants, to a combination of factors. They include missteps by proponents, the lack of public education, and racism. He

undervalues this factor in the hostile response to black American claims and does not consider why race functioned differently, and was not a deterrent, to Japanese Americans receiving reparations for being unlawfully interned during World War II. The case study of Japanese Americans is presented as a model for restitution cases, made possible by a political process that took place in Congress and among constituent groups and allowed for the separation of the moral argument from the actual monetary payment of 20,000 dollars each to survivors and their descendants. It gave both proponents and opponents something positive to respond to, affirming the viability of the win-win strategy.

In subsequent chapters dealing with World War II-era cases, Barkan dissects the politics of Japan’s tortured and limited acknowledgement of guilt in the use of Korean “Comfort Women,” who were forced into sexual slavery by Japanese soldiers. It is the only case discussed in which victimization was based on gender. Yet Japanese resistance to accepting blame energized the victims and left Japan open to international censure. In another World War II-related case, prosecution of Russian capture of German art stalled because the Russians, although guilty of theft, were able to portray themselves as victims of the Nazis. In the case of Eastern Europe, restitution became a *primary focus of postcommunist identity for new governments*, each with a long list of grievances against former rulers complicated by the question of who is most deserving in a region where so many different groups have suffered for so long.

Part two, the struggle of disenfranchised people—Native Americans, Hawaiians, aboriginals, and African Americans—presents different sets of factors that revolve around land restitution, cultural autonomy, and slavery and discrimination. The Hawaiian case, although involving the United States’ transgressions against the native people and overthrow of the island’s monarchy, shows that Hawaii is part of American history. Yet its details have been slighted in American history textbooks. This situation leads Barkan to call Native Hawaiians “the other native Americans.” Yet despite the absence of a popular mandate, Barkan reports that Hawaiians are close to having their claims of land rights and “sovereignty” partially adjudicated.

Together, the United States, Germany, Australia, New Zealand, Russia, and Japan form a catalog of accused nations that have been forced to acknowledge their misdeeds and injustices against minority groups and to enter into dialogue that may or may not result in restitution. It is a process that is buoyed by a will to make human rights part of international diplomacy and may ultimately redefine national identity. Barkan’s cobbling together of these cases also may make us consider not only the consequences of our nations’ past actions but also an acceptable standard of international morality that does not rest on the mantra of generational ignorance: “We cannot be held responsible for events that preceded us.” As Barkan forcefully

concludes, "we enjoy the riches of our past and therefore should pay our historical debts."

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DAVID T. COURTWRIGHT. *Forces of Habit: Drugs and the Making of the Modern World*. Cambridge: Harvard University Press. 2001. Pp. viii, 277. \$24.95.

This book had its origins in an airport duty-free shop. Killing time between flights, David T. Courtwright wondered why he was surrounded by psychoactive products. Ten years later comes this book on "psychoactive commerce" (p. vii).

How did "the big three" (alcohol, tobacco, and caffeine) and "the little three" (opium, cannabis, and coca) become items of global commerce? These developments were part of the expansion of ocean-going commerce; plants, animals, and diseases confined to one hemisphere spread to others. The arrival of tobacco in Europe was part of the "Columbian exchange" that brought Old World disease to the New. The opium poppy spread from Central Europe down into the eastern Mediterranean much earlier, from about 1600 B.C. Its widespread use in the East rested on proximity to supply, medical utility, conformity with religious norms, cheapness, and frugality (it enabled the poor to do with less food). Cannabis and coca also spread globally.

But other drugs did not: kava and betel, for example. Why was this so? For drugs to attain global status, first they had to attract Europeans as medicine, recreational substance, or trade goods. Courtwright suggests plausible reasons why some did not. Luck, politics, finance, elite preference, and even marital alliances—as, for example, between the chocolate-mad Spanish Habsburgs and the Bourbon court—explain why substances did or did not become popular. Substances that have not made it historically could still do so. In 1969, a new products task force assembled by the J. Walter Thompson Company suggested that the tobacco company Liggett and Myers consider manufacturing "betel morsels"; the idea was not taken up. Now such plant products have to compete in a more unequal market, as synthetic drugs and the pharmaceutical industry provide more attractive competitors.

Medical usage gave place to wider recreational use. Amphetamines were a particular example. Their first commercial use was as a decongestant, but by the 1950s, after the end of the first amphetamine patents, an "amphetamine democracy" included long-haul truck drivers, students, and celebrities. John F. Kennedy was injected with Dexedrine before his television debates with Richard M. Nixon. Writer James Ellroy took Benzedrex inhalers with root beer. Addiction as a concept is a conceptual, cultural, and scientific minefield, and Courtwright carefully sidesteps some of the more detailed debates to take a middle-of-the-road position. He sees exposure as a critical

precondition. But the appeal of drugs lay in their social utility as well as their impact on the brain. Culture shaped drug use and it shaped culture in return, with reinforcement from all sorts of ceremonial practices, from convivial toasts to coffee breaks. Drug use generated economic "externalities," many of them profitable. There were associated consumer products like jewelled snuff boxes, and "problem profits," with the provision of lucrative treatment for drug and alcohol abusers. Courtwright draws our attention to the ability of capitalism to betray our senses with one class of products and then sell us another to cope with the damage.

By 1885, taxes on alcohol, tobacco, and tea accounted for close to half of the British government's gross income. Governments in many countries have subjected drugs to any number of different taxation regimes. Farms, monopolies, and other schemes flourished in what Courtwright calls the "golden age of drug taxation" (p. 155) from the seventeenth through to the nineteenth century. Governments make trade-offs here between revenue and public welfare, and also between taxation that is too high or too low. The "high-low problem" still applies, as the current discussions over tobacco smuggling indicate.

During the nineteenth century, elites shifted from their reliance on drug revenues and became more willing to institute controls and prohibitions. By the interwar years in the twentieth century, so Courtwright argues, the landscape had changed. Free trade was increasingly rare, and laws forbade the sale of cigarettes to minors, narcotics without prescriptions, or alcohol after hours. International treaties limited the narcotics traffic and alcohol sales in Africa. Was this an inevitable manifestation of modernity? In Russia, Mikhail Gorbachev's attempts at "modernization" and restriction were defeated by the cultural embedding of alcohol. In other countries, the normality of tobacco use is on the wane, and it is becoming a losers' drug, like opium before it. The relationship of formal policies to economy and culture is complex; what matrix of issues makes restrictive policies enforceable?

Courtwright does not see a way forward in a return to free trade or minimally regulated systems of control. Tobacco and illicit drugs may be on a path to convergence. The future is difficult to predict, given changes in culture, economy, and technology. Whatever the coming state of play, this book is an invaluable and brave attempt to synthesize global historical analysis of a complex field.

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THOMAS R. DUNLAP. *Nature and the English Diaspora: Environment and History in the United States, Canada, Australia, and New Zealand*. (Studies in Environment and History.) New York: Cambridge University Press. 1999. Pp. xv, 350. Cloth \$59.95, paper \$19.95.

As its title and, especially, its subtitle indicate, this is an ambitiously conceived book. Its subject is the relation between scientific understanding and attitudes toward nature in the minds and practices of the "Anglo" settlers of North America and the South Pacific, from the late eighteenth century through the twentieth century. Thomas R. Dunlap uses both intellectual and political developments to periodize his study. Thus, in his view, the nineteenth century up to about 1880 was characterized by vigorous territorial expansion, with natural history as the dominant mode of understanding; the next fifty years by digestion of and identification with these lands, guided by laboratory science; and the first half of the twentieth century by debate about appropriate human power over nature, and the emergence of ecology. The final section of the book discusses the influence of ecological science on the environmental movement of the last decades of the twentieth century. In addition to surveying broad trends, Dunlap focuses on a selection of significant episodes, such as the American and Australian attempts to cultivate semi-arid landscapes, the establishment of national parks and game reserves, and the introduction and partial suppression of alien flora and fauna.

Dunlap has been as ambitious in defining his persona as in choosing his topic. Although most of the book reads like a standard work of scholarship, occasionally a rather different voice breaks through. This alternative voice is likeliest to emerge at the most conspicuous points. For example, the introduction begins (after an epigraph that quotes a "Crow elder"): "We Anglos—whites, whitefellows, pakehas" (p. 1), and the last full chapter (before the epilogue) asks "Where are we now, as we seek to use ecology to hear the spirits of the land?" (p. 305). A parallel framing device is an impressionistic imaginary tour of the lands that are Dunlap's subject, in their precontact condition near the beginning and in their present condition near the end. By thus stretching the conventions of historical prose, Dunlap attempts to redefine his relation to his subject. He acknowledges his own (and his readers') implication in the processes that he describes. Further, by signaling his sympathy with a somewhat mystical strand of contemporary environmentalism, he positions himself politically within his account.

Dunlap's expansive project presents many challenges. All his key terms—science, nature, and Anglo—are slippery and remain so, despite being defined at length in the introduction. (He does not, however, define "English," which he uses as a synonym for "British.") They are problematic in different ways. The difficulties posed by "nature" and "science" have been the focus of extended scholarly debate. The difficulties posed by "Anglo," on the contrary, are underexplored. There is no general label that includes all four of the settler societies that Dunlap discusses, and only those societies (since he explicitly excludes a significant number of Alfred W. Crosby's ecological imperialists). He rather weakly justifies his use of "Anglo" on the

grounds that it is "no worse a cultural tag than most" (p. 4), which may well be true. But it is a distinctively American term, and it does not mean the same thing in all parts of the United States. Its use points to an aspect of this study that is not directly acknowledged in Dunlap's explanation of its design. Rather than offering a fully developed comparison of all four settler societies, the book focuses primarily on the United States and secondarily on Australia. Although Canada and New Zealand provide occasional examples, Canada ordinarily figures as an extension of the United States, while New Zealand is dismissed as having too small a population to support a critical mass of thinkers or actors. A noticeably subdued figure is cut by Great Britain, which also experienced significant changes in attitudes toward its land and other natural resources in the course of the nineteenth and twentieth centuries, and which functioned as the cultural metropolis of the settler societies for most of that period.

It is inevitable that a project with such a broad chronological and geographical range would present organizational challenges. Dunlap has both enhanced his account and exacerbated these challenges by using literary and artistic sources as well as those related to political and scientific activity. His examples from fiction and poetry are persuasive and well chosen, but they tend to be presented in isolation from the discussion of political and scientific developments, thus underlining a sense of fragmentation that characterizes the book as a whole. The scientific and nonscientific wings of Dunlap's argument are developed more or less independently. Similarly, the accounts of events within the various societies are juxtaposed, but not integrated. Even as separate stories, however, they provide much food for reflection.

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FERENC MORTON SZASZ. *Scots in the North American West, 1790–1917*. Norman: University of Oklahoma Press. 2000. Pp. xvi, 272. \$29.95.

This engaging but conventional book seeks to isolate for the reader those aspects of Scottish immigrant culture and enterprise that exerted a significant influence on the history and development of the nineteenth-century West, both in the United States and Canada. In it, Ferenc Morton Szasz, whose previous work dealt with the Manhattan atom bomb project during World War II, presents a series of vignettes concerning the impact of Scottish explorers and fur traders, sportsmen, actors, clerics, shepherds, and cattlemen on the region's development. His most in-depth chapters deal with the dominating influence Scottish explorers and fur traders exerted over the area's economic life in the early nineteenth century, an influence he attributes to the same tough, adventurous spirit that turned Scotsmen into successful empire builders, and with the more long-lasting impact they had on the Canadian, as opposed to the American,

frontier. He attributes the latter partly to the larger number of Scots who immigrated to Canada, and partly to the absence of any self-conscious attempt on the part of the Canadian government to develop a melting-pot culture, which enabled the Scottish immigrants' contribution to remain more distinctive there than it did in the United States.

These points are well taken, but they are not new. More original is Szasz's chapter on Scoto-Indian marital relations, which were quite extensive among the early fur traders. But he does not discuss how far this connection was due to natural attraction and how far it was due to the unavailability of European women, which is a more likely explanation. On other topics, too, the author shows an unfortunate tendency to emphasize breadth over depth, which adds to the anecdotal character of his book. In chapter seven, for example, he lists eight reasons why the Scots were especially attracted to western Canada. But he leaves himself too little space to do any of them proper justice. And he suggests, without providing any historical evidence, that it was the example of the 1746 Battle of Culloden that encouraged Indian sympathizer Alex McGillivray to try to unite the Creek Indians against American repression (pp. 63–66).

Szasz's overall thesis is also not entirely convincing. Early in the book he suggests that it was the Scottish heritage of wandering, and their skills as sheep drovers, miners, and granite carvers, that enabled them to succeed in the West. These arguments make sense. But he then adds that the Scottish talent for gardening, for training ministers of religion, and for inculcating respect for formal education also made them successful. It is not easy to see why. The author takes no account of such recent advances in immigration historiography as border studies and the circular migration of north Europeans in the Atlantic world. Altogether, this book provides some charming anecdotes about the influence of Scottish immigrants in North America. But it adds very little that is new to serious scholarship.

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ALICE BULLARD. *Exile to Paradise: Savagery and Civilization in Paris and the South Pacific, 1790–1900*. Stanford: Stanford University Press. 2000. Pp. 380. \$55.00.

Previous scholars have written about the lot of the people who rose up in revolt in Paris in March 1871, the Communards, 4,000 of whom were deported to New Caledonia, while others have written about the interaction of France and the indigenous Kanak peoples. Alice Bullard is the first to seek a connection between the political deportees and the Kanaks on this Pacific island.

The fruit of the linguistic turn, Bullard's study emphasizes the degree to which both the Communards and Kanaks were viewed as "savages." Under firm rule overseas, each group's members were to be trans-

formed into civilized beings, although the means used were far from civilized. The Kanaks were stripped of their land, forced into subjugation, and, when protesting their condition, killed in various military expeditions. The Communard deportees were beaten, starved, tortured, and, in many cases, worked to death.

Because both were viewed as savages and both were victims of the French authorities, Bullard would like to see these groups as objective allies. Yet when the Kanaks rose in rebellion in 1878 in protest against their loss of land, the Communards rallied to the side of the French administration. This reaction never struck anyone before as anomalous, and probably for good reason; the Communards, as Bullard herself nicely displays, were strong patriots, motivated by love of their nation. With few exceptions, the Communards, like other Frenchmen, viewed the Kanaks with revulsion. They feared them, and when the revolt broke out there was no reason to assume that the Kanaks attacking Frenchmen would differentiate between administrative personnel and the Communards. By siding with the administration, the Communards, Bullard suggests, redeemed themselves in the eyes of the penitentiary authorities and were seen as capable of re-entering civilization.

The sequence of events might suggest that the support the Communards gave the French authorities in the Kanak rebellion led to the amnesty in 1879. But even Bullard does not imply that; instead, she accepts the traditional story that it was the increasing electoral progress of republicanism in France that led to the adoption of the amnesty law, allowing the surviving Communards to return home. That seemed more important than the linguistic turn that made the deportees no longer "savages."

Perhaps the reason that the Communards were no longer tarred with the brush of savagery was not because of anything they did as much as a change of political personnel in Paris; the paranoid Conservatives of 1871 gave way by 1879 to the moderate Republican Opportunists, who instead of labeling the Communards as savages employed the term for the sadistic prison guards and their indifferent superiors.

There are a number of surprising, unsupported statements one would like to know more about: for instance, the assertion that sexual connections between French men and Kanak women were viewed as scandalous by the French. Elsewhere in the empire, certainly, the only Europeans who were scandalized, up to the 1920s, were the missionaries. Repeatedly the book asserts that the vision the French had of the Kanaks was strongly gendered. But it is far from clear that this generalization will hold up. Communarde Louise Michel had a more positive attitude than most men. But Bullard has a number of men, for instance Maurice Leenhardt, also revealing considerable sensitivity toward the inhabitants of New Caledonia. Can we prove that Michel's attitudes were based on her being a woman, or rather on her being a highly unconventional thinker who was capable of putting aside hoary

stereotypes? Most European women overseas, in the face of the “other,” seem to have been Europeans before they were women.

Finally, the book asserts that the understanding of “deep time” based on archeological evidence of prehistory unhinged the French, facing them with an “abyss of meaninglessness” (p. 12). Bullard speculates that, as a result of Darwinism replacing the Genesis account, “the prospect of the savages harbored by deep time must have been extremely frightening” (p. 21). Maybe today such knowledge would cast us in despair, but the French appear not to have been much disturbed by Darwinism; in fact, for decades the French failed to understand Charles Darwin’s originality, clinging to Jean-Baptiste Lamarck’s views, which fit in better with a rational, optimistic view of human nature. One can safely write about French thought in the nineteenth century and ignore Darwin, but not Lamarck, who did not make it into this book.

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ANTOINETTE BURTON, editor. *Gender, Sexuality and Colonial Modernities*. (Routledge Research on Gender and History, number 2.) New York: Routledge. 1999. Pp. xiii, 232. \$55.00.

Modernity is an elusive concept, and it is this elusiveness that animates Antoinette Burton’s excellent anthology of new work on gender, sexuality, and empire. Taking its colonial vocabulary from Mary Louise Pratt and Ann Laura Stoler and its gender cues from Joan W. Scott and Judith Butler, the book never resolves what colonial modernities may be or why such a rubric abets gender analysis. Yet San Francisco’s Chinatown and Niagara Falls tourism vie for space alongside Hong Kong, Canada, Ceylon, India, and Marseilles. Indeed, a good part of what dazzles here is the spatial range, the insistence on interconnected spaces and the mobility of persons and discourses among these contact zones. While British imperial politics are predominant, the book includes the racial and gender politics of French, Dutch, American, Canadian, and Australian colonialisms. Africa, quite interestingly, is present only through Marseilles.

Many themes that we have come to expect for gender, empire, and sexuality studies—domesticity, hygiene, degeneration, motherhood, and venereal disease—are present, as are the reformist campaigns of white colonial women. Thus, we find Anglo-Australian women campaigning to end Aborigine child removal (Fiona Paisley); contagious disease legislation in Hong Kong and the Straits (Philippa Levine); and home science education in interwar British India (Mary Hancock). Yet this is not just more of the same. The essays presented disrupt our expectations about location, reorder tired thematics, and draw attention to the complex entanglements between emerging indigenous

feminisms and local nationalisms. Hancock even gives a gender critique of the concept of public culture.

Significantly, Burton’s organization of the volume pries apart the usually linked themes of hygiene and domesticity. The former is realigned with work on the body, sexuality, and medicine in a section on the mapping of colonial terrains. It combines Hong Kong anti-venereal disease campaigns (Levine) and white feminist-organized hygiene training for Chinatown mothers (Nayan Shah) with the surprise of Angela Woollacott’s essay on the sexual modernities of Australian women living in London in the early twentieth century. This essay adds the themes of European modernity à la Walter Benjamin and Georg Simmel—pleasure, the city, never mind female *flâneurs*—to the collection’s reach.

Burton’s collection also breathes new life into domesticities by combining them with the themes of nation and citizenship. Thus we learn about the Hindu woman’s question in Canada (Ehakshi Dua); pro-indigenous feminist politics in Australia (Paisley); Indian home education science as “a site of hybrid modernity” that accommodated both Gandhian nationalisms and Euro-Western feminisms (Hancock); and the hypermasculinist cowboyism of Indonesian nationalists refusing to be domesticated any longer (Frances Gouda).

Perhaps most original is the section on “Spectacles of Racialised Modernity,” which takes on race, representation, and cultural production through essays on the history of Indian tourist spectacles at Niagara Falls (Karen Dubinsky); Indian postcards as a complicated sexual and political economy that implicated European women as consumers and collectors (Saloni Mathur); and modernist literary writing about interwar Marseilles. In the last, Yaël Simpson Fletcher also turns to Benjamin, contrasting his impressions of colonial migrants and racialized sexuality in Marseilles with those of the Jamaican writer, Claude McKay.

A final section broaches hybridity and negotiated identities. It brings together three south Asianist essays, one on how Sinhala Buddhist nationalists ridiculed bourgeois Christian Sinhala women for being too “modern” in their dress and politics (Malathi De Alwis), which remains too focused on “articulations of modernity and tradition”; one on the Theosophist Society as a “colonial syncretic,” as evidenced by how its World Mother movement was differently translated for Western feminist and Indian audiences (Joy Dixon); and a third on how liberal, middle-class Indian feminists led debates over child marriage, realigning the stance of Indian nationalists on modernity in the process (Mrinalini Sinha).

The concept of “colonial modernity” is the defining structural analytic of the volume, while there are many kinds of modernity—European, postcolonial, nationalist, subaltern, universalist, racial, sexual, syncretic, and differential—included. It is a taste for multiplicity and difference, precariousness and vulnerability, porousness and faultlines that prevails here. The key

point is that colonial modernities were always in process, always unfinished.

This book will serve as an excellent text for graduate seminars, though it might have joined the modernity bandwagon less and problematized its implications more. But considerable materials—both theoretical and empirical—are there to engage these questions, especially within the implicit contrast that is suggested by these essays. Europeanist and subalternist historians of gender and empire may be speaking to each other more than they used to, but they seem to be taking up modernity as a historical and theoretical predicament in strikingly different ways.

NANCY ROSE HUNT
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M. CRISTINA ZACCARINI. *The Sino-American Friendship as Tradition and Challenge: Dr. Ailie Gale in China, 1908–1950*. Bethlehem: Lehigh University Press. 2001. Pp. 228. \$41.50.

Academic authors increasingly cannot rely on good copyediting by publishers. This unfortunate development is evident in M. Cristina Zaccarini's oddly titled book. Ailie Gale was an American doctor attached (by marriage) to the Methodist Episcopal mission in China; she was not technically a missionary herself. Gale was in China during tumultuous years for foreign missionaries and maintained a voluminous correspondence with home supporters of missions. This correspondence forms the basis for Zaccarini's book.

Zaccarini proposes two primary arguments, both of which are problematic. Her first argument is that women missionaries represented an alternative both to feminism and to domesticity. She notes that "many women spoke in a language which advocated an increased role for women, justified by piety" (p. 16). She calls this "empowerment-through-piety," or "cultivation-through-piety." Zaccarini asserts that "secular feminist scholars" have ignored this phenomenon "due [to their] need to maintain practical categories of analysis" (p. 15). Leaving aside the troublesome appellation "secular feminists," this claim flies in the face of fifteen years of historiography. Women's historians have long been aware that many nineteenth and twentieth-century women used religious piety as their justification for work and activism outside the home while specifically eschewing feminism.

Moreover, in the case of Gale, it seems clear that it was competence as a doctor, not Christian piety, that empowered her, in China and in the minds of her home supporters. Zaccarini herself makes this point: "Pride in her professional accomplishments reflected Gale's belief in an order where competence was a source of individual power regardless of gender or race" (p. 150). Two pages later, however, Zaccarini again cites Gale's "[b]elief in empowerment through piety" (p. 152).

Zaccarini's second argument is that through letters to mission supporters in the United States, women

missionaries shaped Americans' sense of friendship toward China. This is not a new argument either. Zaccarini's emphasis on the role of women missionaries is intriguing but unsupported. While Gale's letters undoubtedly helped home supporters form an impression of China and their country's relationship to it, since Zaccarini cites few letters from these individuals to Gale it is impossible to ascertain what that impression was, or how Gale's portrayal of China was interpreted. Zaccarini further asserts that, in contrast to the feminizing image of China that other scholars have posited in missionary writings, Gale "redefined the term 'female'" (p. 151). Apparently, being "heroic soldiers" and having "astute military strategies" (p. 152) were now female attributes. This is not convincing.

Other contradictions and problems abound. Zaccarini mentions "Victorian mores against public speaking" (for women) in 1902 (p. 80). Few historians would accept that the Victorian era stretched into the twentieth century, or that women's public speaking was still an issue. Gale "was a cultural imperialist" because she insisted that "the Chinese needed to replace their religious beliefs with Christianity" (p. 135), but she "accepted traditional Chinese religions because she had been flexible in her interpretation of Christianity" (p. 169). She had "always been the dutiful wife" (p. 176), even though she did not raise her own children or perform domestic tasks (p. 93) and taught her adopted daughter that "public service work . . . took priority over marriage" (p. 132).

Rather than straining to make an original argument based on Gale's letters, it would have been better for Zaccarini to have edited and annotated those letters. Gale was not, in fact, a particularly astute observer of the Chinese scene, as Zaccarini admits; still, being able to read her letters in full might be of value to historians. Alternatively, Zaccarini might have written a more tightly argued article on some theme in Gale's correspondence, perhaps analyzing the contradictions in Gale's writings. For example, Gale's ideas about racial hierarchy are conflicted. While she seemed genuinely concerned about the well-being of the Chinese (both the poor masses and her own Chinese colleagues), she nevertheless consistently represented herself as superior. Was this because of her medical and administrative skills, or because of her race? Was it simply a way of reassuring her supporters that their money was well spent? Gale was also unusual in being a married medical doctor in a time when most women had to choose between marriage and a career. An article examining how she perceived her own status and how she presented it to others could be quite interesting. Did Gale believe that women could "have it all"? If so, what are the implications for the history of feminism and domesticity? As it is, historians will not find Zaccarini's study illuminating.

GAEL GRAHAM
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KEVIN RUANE. *The Rise and Fall of the European Defense Community: Anglo-American Relations and the Crisis of European Defense, 1950–55*. (Cold War History Series.) New York: Palgrave. 2000. Pp. ix, 252. \$65.00.

As Western leaders shifted to perceiving the USSR as more threatening than Germany, they concurrently sought via economic and military/political integration to dampen the intra-European rivalries that had sparked two world wars. But Anglo-American determination to bolster European defense by rearming Germany intersected awkwardly with the quest for supranational decision-making institutions. French pride, insecurity, instability, and indecisiveness challenged German rearmament and supranational defense institutions, with potentially disastrous consequences for European defense and transatlantic relations. Kevin Ruane's extended case study discusses how British and American leaders responded to French reluctance to embrace the European Defense Community (EDC) (and even to vote on it), to the EDC's eventual rejection, and to one another's responses. U.S. Secretary of State John Foster Dulles had threatened an "agonizing reappraisal" of military deployment should France reject the EDC. British policy makers recognized that while German manpower was necessary, American military capability was even more essential. This book analyzes how Britain—especially Foreign Secretary Anthony Eden—worked to preserve the North Atlantic Treaty Organization (NATO), Western security, and transatlantic relations. Thus Ruane contributes not only to our understanding of the 1954 EDC crisis but elaborates on the scholarship of David Reynolds and others: the reassessment of the mythical "special relationship" continues.

Ruane challenges prevailing wisdom about Eden, Britain, and Europe. Thus while Eden knew Britain could not enter the EDC, he labored to save it. So French fear of German rearmament and diminished sovereignty killed the EDC, not British rejection of France's demand for "a written pledge to maintain a set number of troops in Germany throughout the Community's lifetime" (p. 35). This contention is thoroughly supported by reference to other factors encouraging French dilatoriness: the Fourth Republic's weakness, the Indochina War that reduced France's military and political weight in the EDC, the Saar's future, and Joseph Stalin's death, which sparked disputes over whether first to finalize defense arrangements or conduct summit diplomacy (pp. 36–40).

Britain's effort to save the EDC was rooted in Anglo-American relations—driven by "Atlanticist imperatives" (p. 183). Ruane contends that "Washington's fervent support for a United States of Europe, and for the EDC as an essential adjunct of that aim, had a significant impact on British policy" (p. 25). Thus "Eden's support for the EDC owed far more to the American factor than to any faith in the project's military efficacy" (p. 8), especially once Dwight D.

Eisenhower clarified at Bermuda in December 1953 that he preferred redeployment to peripheral defense to an intergovernmental approach like German entry into NATO. American pressure failed to move France but nudged Britain into action. "Rather than continue to anticipate in its collapse an opportunity to put forward an intergovernmental plan for German rearmament, Eden would seek . . . to do everything possible to ensure the EDC's success. This represented neither a belated recognition of the EDC's military worth, nor a sudden acceptance of supranational ideas of European unity, merely a determination to deny the Americans any pretext for acting upon their threat to abandon Europe" (p. 55). Britain's own "agonizing reappraisal" reversed Eden's prior determination "to go no further into Europe than the Americans themselves were willing to go" (p. 23) and culminated in specific continental military commitments.

Ruane reexamines American pressure, rejecting the view that Dulles's public threat of an "agonizing reappraisal" was a simple bluff. Rather, Dulles moved to preempt Congress: "so committed was the Eisenhower Administration to the Atlantic Alliance and to European security, and so concerned was it that Congress might respond to any slowdown in European integration by reducing aid to NATO, that it found itself compelled to use all available means, including crude coercive diplomacy" (p. 44). Also, Eisenhower "was undoubtedly committed to a measure of ground force redeployment as part of the New Look" (p. 138). So Eden did not overreact to a nonexistent bluff; rather, the U.S. successfully bluffed France into accepting German rearmament under the umbrella of the British formula of adapting the Brussels Treaty's multilateral safeguards to Germany's position in NATO. Thus it was "arguably the threat of America proceeding unilaterally to arm the Federal Republic that was the real bluff . . . [and] the most productive" (p. 187).

This insight typifies Ruane's careful logic. His traditional approach ably deploys pertinent, well-crafted, archivally based quotations from policy makers. Still, he references multiple domestic political contexts without thorough investigation of domestic opinion. Although he mentions the immediately relevant EDC and Anglo-American relations literature, a broader context would have been useful. Thus he notes the American bureaucratic failure when Dulles had "no specific alternative proposal prepared" (p. 141) following EDC's rejection, thereby ceding influence to Britain, without contextualizing this event in historical precedent (e.g. Americans unprepared for a united British delegation at Casablanca in 1943). Despite efforts to provide balance in his conclusion, Ruane's tone reflects the sympathies of his British subjects, especially in his impatience with Dulles and with France. Nevertheless, this elegantly crafted and subtly argued book achieves a thoughtful and significant

reevaluation of the Anglo-American resolution of this crisis.

KEVIN SMITH
Ball State University

ROLF STEININGER. *Der Mauerbau: Die Westmächte und Adenauer in der Berlinkrise 1958–1963*. Munich: Olzog. 2001. Pp. 411.

Of the great power confrontations during the Cold War years, the Cuban missile crisis is probably best remembered and is generally considered to be the most dangerous one. Rolf Steininger rejects this notion. Instead, he agrees with Paul Nitze, assistant secretary of defense during the Kennedy administration, who declared that the Berlin crisis was a more dangerous conflict. Unlike the thirteen-day missile crisis, the Berlin conflict dragged on for more than half a decade (November 1958 through 1963) and left ample room for misunderstandings about how far each side would go to defend its position in the divided city. During the brief but serious Allied tank confrontation at Checkpoint Charlie in October 1961, the Berlin conflict reached an intensity that rivaled the missile crisis. Moreover, while the West closed its ranks behind President John F. Kennedy during the Cuban conflict, the Berlin confrontation was also characterized by difficult and protracted deliberations among the Western powers about how to react to the Soviet challenge. This decision-making process and the underlying deterioration of the relationship among the leading Western politicians are the focus of Steininger's book.

Soon after Soviet Prime Minister Nikita Khrushchev ignited the crisis in November 1958 by demanding a transformation of West Berlin into a "free city" within six months, disagreements within Western ranks became obvious. The West German government was aghast and declared that the West had to stand firm. There was no room for negotiations over Berlin, particularly not under the threat of a Soviet ultimatum. The British government, in contrast, appeared willing to negotiate with the Kremlin. Prime Minister Harold Macmillan feared that the West might soon be confronted with the alternative of accepting a Soviet fait accompli in Berlin or starting a war. London, therefore, favored the idea of an Allied summit on Germany, a prospect that was extremely unappealing to the Bonn government because it would put the fate of Germany again in the hands of foreign powers fifteen years after the end of World War II. In the wake of Macmillan's February 1959 trip to Russia, Steininger characterizes the relationship between him and Chancellor Konrad Adenauer as mutually distrustful. Adenauer spoke of a "British weakness," and Macmillan called Adenauer a vain, old man.

The summit, scheduled for May 1960 in Paris, was canceled, however, at the last moment by the Soviets after the U-2 incident. Khrushchev, it appeared, waited for President Dwight D. Eisenhower's succe-

sor before he would renew the Berlin challenge. Kennedy immediately made clear his determination to stand firm on maintaining the United States position in West Berlin. But he also let the Soviets know via the U.S. embassy in Moscow that he would not retaliate militarily if they signed a separate peace treaty with East Germany, as long as Western rights in the city remained unaffected. By building the wall, the Kremlin implicitly gave up all earlier claims that West Berlin was part of the German Democratic Republic (GDR) (as Stalin had demanded during the blockade in 1948–1949) or should be turned into a "free city." To the Western Allies, as Steininger points out, the wall therefore came as a relief. U.S. Secretary of State Dean Rusk declared that the division of the city would make a "Berlin settlement easier." Most West German politicians, however, were deeply disappointed by the West's inaction. Heinrich Krone of the governing Christian Democratic Union (CDU) noted in his diary that it was hard to believe that America would recognize the validity of a border in the middle of Berlin.

Steininger's book is a reliable account of the many facets of the East-West and interallied confrontation that make the Berlin crisis a more challenging task to analyze than most other Cold War conflicts. The author emphasizes the Western Allies' dilemma to maintain their position in West Berlin, while trying to reduce tensions with the Soviet Union and (barely) giving lip service to the West German demand for reunification. From today's point of view, the building of the wall appears almost as a precondition to German reunification in 1989–1990: the wall not only prevented the outbreak of hostilities in Europe for three decades, but it also stopped the Western Allies (particularly Great Britain) from pursuing plans for a de jure division of Germany. The wall simply froze the unfinished political structure of Germany until the Cold War was over.

The book is based on a careful analysis of Western government documents, many of them only recently declassified. The author also profited from a Berlin-crisis scholarship that has mushroomed in recent years and led to the publication of numerous detailed studies about the East and West German, Soviet, American, British, and French positions during the Berlin crisis. Steininger manages to synthesize all of this into readable text suitable for historians as well as for the interested public.

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ASIA

SHIH-SHAN HENRY TSAI. *Perpetual Happiness: The Ming Emperor Yongle*. Seattle: University of Washington Press. 2001. Pp. xv, 270. \$32.50.

Emperors of China who ruled before the Qing dynasty (1644–1912) are difficult figures to approach as people. The mystique of the office they held, their disin-

clination to reveal themselves on paper, and their want of friends and social contacts (aside from eunuchs and palace women, who did not or could not write) make extremely scarce the intimate documentation that supports the detailed and vivid biographies that have been written about so many European monarchs. In those very few pre-Qing imperial biographies that have been published thus far, what perforce substitutes for biography is a survey of the reign, with inferences about personal character mainly drawn from the edicts and other official acts as registered in the standard dynastic historical sources. Morris Rossabi's *Khubilai Khan: His Life and Times* (1988) and Charles P. Fitzgerald's *The Empress Wu* (1955) are two well-received examples of the genre. The present account of the Yongle emperor (r. 1402–1424) now joins this select club. "Perpetual happiness" translates the reign-title "Yongle."

Unsurprisingly, this is less a biography than a synopsis of the reign of the third Ming emperor. The Yongle reign is a good choice, because so much took place. In 1399–1402, a civil war was fought and won, as Yongle seized the capital Nanjing from his imperial nephew. Then, in an orgy of activity, the usurper purged his enemies and redesigned government at its top level; threw open the floodgates of opportunity for the takers of the civil service examinations; sponsored literary projects; reasserted the neo-Confucian orthodoxy; dispatched eunuch-led missions to Tibet, Nepal, northern Manchuria, Herat, and Samarkand; sent those famous fleets as far as the east coast of Africa; invaded and annexed Vietnam; and personally conducted five military offensives into Mongolia, on the last of which he died. It was a most spectacular reign.

Shih-shan Henry Tsai has covered all these topics and more in a short volume that seems aimed at the general reader and the advanced undergraduate. The book is well based in sources, both original (such as they are) and secondary. Maps and a selection of photographs accompany the text. The presentation is in the story-telling mode, here and there interrupted by long factual recitals. Inevitably, there are faults. For example, almost every Mongol name has been mangled in spelling. Kōkō Temür was, despite his Mongol name, Chinese and not Mongol (p. 23). The author misstates the story of Yin Changlong's execution because he did not consult the original documentation that survives about the case (p. 74). Little attention has been given to such problems as how the Yongle reign fits in the longer flow of Chinese history, or into the global context of the early fourteenth century. But on the whole, the book makes for a good survey, and I would recommend it as outside reading to students.

JOHN W. DARDESS
University of Kansas

D. E. MUNGELLO. *The Spirit and the Flesh in Shandong, 1650–1785*. Lanham, Md.: Rowman and Littlefield. 2001. Pp. xiii, 209. Cloth \$70.00, paper \$24.95.

Lively and all too human are the people who inhabit D. E. Mungello's latest work on European missionaries and Chinese Christians during the early Qing years. Like his previous efforts, this one is outstanding and accomplishes what every historian seeks to do: put the reader in the time and place where the story happens. Based on a wide variety of Chinese and Roman Catholic sources, the work is meticulously researched and masterfully written. Not only do the sights and sounds of Qing China come alive here but also the foibles, prejudices, and outright hatreds of missionaries, Chinese officials, and converts. The Franciscan Antonio Caballero's first attempt to enter China from the Philippines in 1633 was viewed by the Jesuits, already working in China, as such a threat that their treatment of him, in Mungello's words "fell considerably short of what might be called Christian love." They provided him with an assistant who was instructed to harass him and then had him kidnapped and held aboard a ship for six months before returning him to the place he had entered China! Remorseful for his participation in these acts, the Jesuit vice provincial later sought forgiveness for his role in the affair (pp. 8, 12). Other disputes followed, including one a century later over the Jesuits conducting too elaborate a funeral for one of their members (p. 91).

Mungello has such command of his sources that he is able to tell us very precise details: that Father Augustinus Paschale arrived in Manila in 1670 aboard a ship owned by Armenian merchants (p. 55), and that orphaned children incarcerated by the authorities in Ji'nan were kept alive only by eating the leftovers of the adult prisoners; upon learning of the children's plight, the very poor local Christians doubled the amount of food they delivered to jailed priests (p. 29).

The difficulty of making the Christian story fit the Chinese model of orthodoxy is detailed in a chapter on the collaboration of Caballero and Shang Huquing, a *juren* degree holder who had been baptized as a young man and who briefly held a minor government post. This collaboration produced several works that sought to synthesize Christianity and Confucianism. Shang's major work, *A Warning to Repair the Deficiencies of the Literati*, is seen by Mungello as addressing some of the fundamental problems Confucian scholars had with Christian teachings, such as their ambivalence toward an afterlife (p. 41).

The presence of the underground church with its secret meetings held to avoid official prohibitions of Christian teachings, along with overlapping membership of converts in quasi-religious secret societies such as the White Lotus, convinced the government that Christianity was indeed a subversive sect that needed to be suppressed. That both priests and converts persisted in their Christian beliefs in the face of arrests, incarceration under unbelievably subhuman conditions, persecution, and martyrdom is a testament to the tenacity of their faith. Indeed, Mungello argues that those villages that converted to Christianity centuries ago still produce Christians today.

At times, the story sounds startlingly contemporary as we learn that two Franciscan priests were involved in sexual activities. Both of these men worked in the underground church in Shandong, secretly traveling about the province making contact with Christians, many of whom had not seen a priest in decades, furtively seeking shelter with converts, and always ready to flee to a neighboring province if the authorities became too inquisitive. Father Bernardino Bevilacqua was involved in what the Chinese termed a “filthy tragedy” when he committed adultery with some women, raped several young girls, and might have had homosexual relations. The case was settled by Father Giovanni Boucher, who managed to convince the Chinese that Bevilacqua suffered from a “temporary case of madness” and demonic possession,” and Bevilacqua, broken both physically and mentally, was sent to Macau to recover (pp. 127–28). Father Alessio Randanini tried to purchase the sexual services of a non-Christian woman, whose outraged family was silenced by the intervention of a prominent Chinese Christian (p. 133).

The book is enhanced by contemporary paintings and maps of the areas where the missionaries worked, and current pictures of the remaining churches in Shandong. In short, the author has produced another page-turner, the scholar-specialist is given much to contemplate, and the casual reader will be treated to a glimpse of the lives of early Roman Catholic missionaries and their converts, whom few can describe as well as Mungello.

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JONATHAN HAY. *Shitao: Painting and Modernity in Early Qing China*. (RES Monographs in Anthropology and Aesthetics.) New York: Cambridge University Press. 2001. Pp. xxiv, 412.

Should the term “early modern” be applied to seventeenth and eighteenth-century China? Those who would say yes have claimed various kinds of connections with European modernity: an entangled global history that encompassed both; comparable stages (experienced at different paces) that lead to the present; or parallel developments that appear retrospectively similar. The latter possibility is explored by Jonathan Hay in this wonderful contribution to Chinese cultural history. Hay has transformed his dissertation into a comprehensive and fascinating history of the artist Shitao (1642–1707). A major painter and calligrapher of great versatility and intensity, Shitao merits the attention of historians everywhere.

An orphaned descendant of the defeated Ming dynasty, Shitao lived through the tough times of early Qing rule. He is characterized in the literature as an early “individualist” painter, and has been rather well

studied by scholars (sometimes under the English name “Tao Chi”). Hay builds on and recasts this existing work. His book consists of ten closely written chapters, full and interesting notes, a substantial bibliography, a detailed chronology of Shitao’s life and his major works, Chinese and English texts of his twenty-five surviving letters, and an index of his artwork. Hay carefully reexamines both the well-established and unusually large corpus of accepted paintings (700 or more), and the poetry and prose that the artist inscribed on them. The book is resonant with quotations from Shitao and his contemporaries and thick with illustrations (twenty-two in color and 220 in black and white across 322 double-columned pages). These rich materials permit Hay to consider Shitao’s life and art in detail, and he does so in prose that is intense and elegant. His interpretations of Shitao’s written and painted work are carefully reasoned and documented.

Hay confidently embeds his artist in the wider political and social context of the early Qing. The first five chapters examine Shitao’s history and early concerns: his unsettled life as a monk and a “Ming remnant”; his clerical friends and merchant patrons; and his emergence as an artist. We also learn, inter alia, about the politics of the Kangxi reign, public and private spaces, and life in the Grand Canal city of Yangzhou at the end of the seventeenth century. The latter half of the book focuses on Shitao’s uneasy metamorphosis from independent monk to dependent professional painter, his transitional identity as a Daoist, and his exhausting and prolific final decade. Here Hay concentrates on Shitao’s art, discussing in passing his teachers, pupils, and studio assistants and the evolution of his well-articulated ideas about painting. These chapters, like the rest of the book, are probing but accessible to interested outsiders. The last chapter, “The Private Horizon,” is particularly absorbing. It delicately examines Shitao’s frustrations with age and illness, his sadness, and his passions. Lush paintings of vegetables and flowers are persuasively read as expressions of physical excitement, and the sheer pleasure that Shitao took in the focused act of painting is vividly conveyed. One begins to understand why it was said that when professional painters were confronted with Shitao’s distinctive, “exuberant improvisatory” style, “Their tongues stick to the roof of their mouth, the light dies in their eyes” (p. 255).

Earlier studies have tended to understand Shitao as a literatus whose authentic self was kept hidden for political reasons. Hay leaves behind this familiar but limited perspective and gives serious attention to the many other, often religious, concerns of this proud, solitary, and gifted man. He argues throughout the book for a much more complicated, fractured, and hybrid self. Hay’s argument for Shitao’s “modernity” focuses on this hybrid subjectivity and on the painter’s doubts, self-consciousness, and aspirations for autonomy. These Hay links to the fluid, risky, and highly commodified world of the early modern Chinese city, a world that will be familiar to Chinese historians from

the work of William Rowe and Craig Clunas (among others), here recast through the ideas of Anthony Giddens. The wealth of insights and information in Hay's book and the complex and believable portrait of Shitao do not, however, depend on this argument about the early modern nature of his subjectivity, but together they should provoke the attention of the wide readership of cultural historians that this powerful artist and excellent book deserve.

SUSAN NAQUIN
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PAUL S. ROPP. *Banished Immortal: Searching for Shuangqing, China's Peasant Woman Poet*. Ann Arbor: University of Michigan Press. 2001. Pp. xvii, 297. \$45.00.

One of the great pleasures of reading and writing history these days is the revelation that what might have once seemed exotic and too alien for ordinary historical investigation is no longer extraordinary. This observation is particularly relevant to the history of literate Chinese women during the late imperial period (1600–1900). To date, the most successful studies of writing women of this era have focused on the urban Jiangnan elite. There, in the sophisticated milieu of Nanjing, Suzhou, or Hangzhou, educated women had the greatest resources to write and publish, and thus leave a record of their accomplishments. We now know that well-bred courtesans who matched wits with their sophisticated patrons, as well as semicloistered women from upper-class families, have generated new voices in Chinese literature since the end of the sixteenth century, and that men began to write about these talented women. But what about female literary culture elsewhere in imperial China? And how did male perceptions of female-authored writing change over time?

In this beautifully written book, Paul S. Ropp explores these questions through an analysis of one man's memoir of a gifted but unknown peasant poet, He Shuangqing, and the evolution of her place in Chinese culture from the eighteenth century until now. What is so unique and penetrating about Ropp's investigation is how he weaves his scholarly quest to authenticate Shuangqing's works and poetic relations with his own personal voyage into the world of present-day Chinese historical and literary scholarship on women and gender. His diary of his 1997 research expedition to Jintan and Danyang, the rural counties in Jiangsu Province (south central China) that were home to Shuangqing, forms the backdrop to his step-by-step examination of her place in China's literature. The reader of this volume experiences at once the satisfactions of scholarly research, verification, reflection, and collegial exchange.

Shuangqing owes her fame in the first instance to a male literatus, Shi Zhenlin (1693–1779), whose memoir *Random Notes from Xiqing* (*Xiqing sanji*) recounts how he discovered this “brilliant” woman poet living in

a poor peasant family in a remote rural village. Shi himself was an obscure scholar, poet, and painter. Despite his success in the highly competitive civil service examinations—he attained the highest degree—and an official appointment as an instructor in a government school, he voluntarily retired from his duties to spend the rest of his life “traveling, sightseeing, visiting friends, drinking, writing, and painting” (p. 47). Ropp attributes Shi's extensive wandering around rural China as an escape from the anxieties and social pressures of the age. The record of his retreat, *Random Notes*, written in stream of consciousness form, gives voice to Shi's fascination with “other-worldly” phenomena (dreams, feelings, spirit writing), his obsession with beautiful women of literary talent, and his meeting with Shuangqing. This work narrates Shi's encounters with her, quotes her verse and song lyrics, and according to Ropp, conveys the ambiguity of her existence. Was Shuangqing a real-life paragon of virtue, respected by Shi and his fellow scholar poets for her self-education, compassion, writing talents, and, not least, her beauty? Or was Shuangqing part of an elaborate “tease” by Shi to vent his frustration with contemporary orthodoxies and social conventions? After reporting his extensive interchanges with local experts and other modern scholars on these questions, Ropp concludes there is no direct way to sort out the contradictory evidence over Shuangqing's existence and Shi's idealization of her.

But it is clear that historians and literary scholars since the time of *Random Notes*'s first appearance have utilized the Shuangqing story for their own purposes, and that changing interpretations of her and her poetry indicate shifting cultural concerns and preoccupations. As Ropp narrates, a number of nineteenth-century literary compilations, edited by both men and women scholars, quoted and commended her to illuminate the achievements of women writers, and to foster pride in Jiangsu's cultural attainments. In the early twentieth century, when intellectuals began to reassess “China's myth-encrusted past in the cold-light of scientific rationality” (p. 235), some scholars dismissed the Shuangqing narrative as the product of a pathetic, narcissistic fabricator, while others saw this work as the triumph of Chinese aestheticism. In Maoist China, not surprisingly, Shuangqing became “a heroine illustrating the genius, virtue, courage, and perseverance of a peasant woman under the old feudal regime” (p. 266). And, finally, nowadays, the story of Shuangqing and Shi Zhenlin may prove to be a saga “made to order” for “argument about the ways cultures are constructed” (p. 267).

In conclusion, Ropp has given us a very readable, persuasive book with an important message: the history of “people on the margins” matters not just for what it tells us about their particular circumstances but also for what it says about how we write that history.

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RYAN DUNCH. *Fuzhou Protestants and the Making of a Modern China 1857–1927*. (Yale Historical Publications.) New Haven: Yale University Press. 2001. Pp. xxi, 293. \$35.00.

Ryan Dunch's work challenges the traditional views of the roles of Christian converts and their reception by non-Christians in late nineteenth and early twentieth-century Chinese society. Rather than seeing them as peripheral and largely ignored, if not scorned, this study places them near the center of political and social life in the treaty port of Fuzhou, where the local community was open to their participation. There the mission work of three bodies—Methodists, the American Board, and the China Inland Mission—as well as the Young Men's Christian Association (YMCA) produced converts who were, according to Dunch, motivated by a Protestant model of "moralistic nation-building." He sees the first two decades of the twentieth century, a period of flourishing voluntary associations and local projects, as one where "the birth and articulation of a vision of a Chinese modernity" grew "organically from local civil society" (p. 198). The possibility of further development of this civil society was nipped in the bud by the Nationalist Revolution in the 1920s.

Dunch's focus on Chinese Christian converts rather than on missionaries makes his book unique in this field where major studies on the role of Christianity have looked primarily at the roles and initiatives of missionaries. He elucidates the conversion process by showing that converts accepted their new Christian beliefs in part at least because they could be fitted together with Chinese traditional religious and ethical "sensibilities." While, early on, supernatural beliefs and images were most important, in the twentieth century the Western origin of Christianity made themes like science and progress paramount in potential converts' choice to become Christians.

A major question, as in every case study, is the applicability of the Fuzhou findings for the greater Chinese picture. More case studies of the roles of converts over time should provide an answer. Dunch has done an admirable job of ferreting out much fragmentary data and making as strong a case as possible for his thesis. The numbers of graduates of the Anglo-Chinese College, many of whom left Fuzhou, certainly played many modernizing roles beyond the city. But in Fuzhou itself, the very small number of influential Protestant converts that makes up the analyzed population is troubling.

For the first twenty-five years of the twentieth century, I could count only twenty-two Protestants who played roles as any kind of leader—this in a city that G. William Skinner posits already had a population of 300,000 in the 1840s. More specifically, one of twelve founding members of the voluntary reform society the Yiwenshe was Protestant; six of the eighty-seven-member Qiaonan reform society were, with nine others having "known Protestant ties," whatever that

specifically means. Two of the seventy-five-member provincial assembly were Protestant, though one was an assembly vice president. Three Protestant men headed departments in the immediate post-1911 provincial government, yet they were rather quickly dismissed once the permanent postrevolutionary government was established.

In discussing the acceptance of Protestants by progressive elites, the author admits that "we cannot say precisely whether their acceptance as part of the progressive elite was due to their religion, incidental to their religion, or in spite of their religion" (p. 77). If the reality might be either of the last two possibilities, then it seems to me that the relevance of the Protestant identity to the political inclusion of these relatively few leaders is called into question.

Perhaps for this reason, the author seems at times to set up straw men to set his work apart from others. For example, he states (p. 87) that assemblyman Huang Naishang "is a perfect illustration of the danger of imputing the class character or motivations of provincial assembly members on the basis of formal attributes such as age, degree rank, or place of residence, *as so much of the literature has done*" (emphasis mine). But the endnote, far from pointing out any sizable range of literature, notes only one work.

In the end, in line with his revisionist interpretation, Dunch rightly concludes by cautioning against essentializing those themes and binaries crucial for interpreting Chinese society in the twentieth century. Nationalism drew on multiple currents, including both resistance and receptivity to the West. But perhaps more to the point, in this study, "Chinese" and "Western," far from being distinct, were "constantly overlapping and interpenetrating" (p. 196).

R. KEITH SCHOPPA

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WEILI YE. *Seeking Modernity in China's Name: Chinese Students in the United States, 1900–1927*. Stanford: Stanford University Press. 2001. Pp. xii, 330. \$49.50.

This is a fine book, one that makes a signal contribution to early twentieth-century Chinese social and cultural as well as intellectual history. Weili Ye has revisited an important cohort of Chinese intellectuals, insightfully analyzed them, and, perhaps most important, engagingly brought them to life for us. This work is thoroughly researched, clearly documented, and convincing in interpretations. The writing style is spotty, occasionally suffering from overstatement but often quite eloquent.

The Chinese students in the United States in the first three decades of the twentieth century long have been overshadowed (among historians) by those who studied in Japan or Europe. Students from the latter two places played a bigger political role in the development of the 1911 and Nationalist revolutions and the Communist Party. Student returnees from the United States were looked at by a few scholars some

years ago, but they have been characterized as rather elitist, individualist, reformist liberals, alienated both from the central corridors of political power and from the realities of grass-roots China. This has been the overall interpretation of Y. C. Wang, *Chinese Intellectuals and the West, 1872-1949* (1966), and Jerome Chen, *China and the West: Society and Culture, 1815-1937* (1979), among others. Ye does not so much claim that this view is untrue as show it to be far too limited an assessment. The early generation of Chinese with experience in the United States, since the 1950s long reviled as "bourgeois" in the People's Republic of China, has (now that they are all dead) been rehabilitated in recent years as important contributors to China's modernization. But still more important is Ye's claim, of which she soon convinces the reader, that while some Chinese intellectuals by the turn of the century (e.g. Liang Qichao) were convinced of the need for China to become "modern," this unique group of young Chinese who came to the United States actually lived out that quest for modernity in their individual personal lives. In her telling, this turns out to be a fascinating and often poignant story.

The book has six substantive chapters, starting with two related ones on student associational life in the United States (mainly the Chinese Student Alliance) and the beginnings of professional associations, all in the context of rising Chinese nationalism and, by the 1920s, disillusionment with China's political chaos. The next three chapters are just outstanding: on race, women, and the tension between "morality and romance," respectively. Ye believes, contrary to Frank Dikötter (*The Discourse of Race in Modern China* [1992]), that race was a modern concept, one that confronted Chinese intellectuals because of Western imperialism and that confronted these particular Chinese students especially acutely because of the U.S. exclusion laws and racial climate. In the chapter on women, Ye actually goes beyond the U.S. cohort (though it remains central) to provide a profile of women's issues from the 1880s to the 1920s. She supplements her analysis with capsulized accounts of a few specific individuals, a technique she uses effectively in most chapters.

Chapter five delineates with great insight and sensitivity the personal emotional dilemmas and choices of this generation, including gender relationships, interracial dating, love and sexuality. All of these explosive issues were confronted in the context of a rapidly changing U.S. society and with the pull of family obligations and Confucian morality still tugging at them from China. The subsection on Hu Shi's romance with the American Edith Clifford Williams is exceptionally well done. In my view, this chapter is the best in the book; it reminds me of some of the unforgettable personal portraits in Jonathan Spence's *The Gate of Heavenly Peace: The Chinese and Their Revolution, 1895-1980* (1981).

No book is perfect. The last chapter, on physical education and theater, seems flat in comparison to the

rest of the book, and the epilogue, tracing the post-1930 careers of three members of this generation, ends abruptly with a very thin conclusion. Ye also minimizes the Christian identity of this generation and the extra complexity that factor brings to the story. She recognizes that a large number of them had been educated in mission schools in China and duly notes that the second largest association among the students was one for Christians, which may have enrolled half or more of all the students, but she devotes only half a page to this important group. Finally, the glossary of Chinese characters has only persons' names, omitting the many important terms used in the text. Nevertheless, this is an absolutely first-rate work, for which Ye should be congratulated.

DANIEL H. BAYS
Calvin College

HENRIETTA HARRISON. *The Making of the Republican Citizen: Political Ceremonies and Symbols in China 1911-1929*. (Studies on Contemporary China.) New York: Oxford University Press. 2000. Pp. viii, 270. \$74.00.

What makes the Chinese Chinese? This important and pioneering study argues that modern rituals of statehood created a new political culture in the early twentieth century that, in turn, produced lived practices of everyday life that finally created a new sense of Chineseness. By focusing on rituals and symbols that simultaneously connected and differentiated political elites and the populace, Henrietta Harrison's methodology links issues normally associated with ethnicity with those of national citizenship.

Harrison traces a process whereby various groups, including but not limited to state actors, created an image of modern Chinese citizens: short hair for men, natural feet for women, handshaking, the solar calendar, and new marriage rituals and clothing. Harrison shows how even ordinary urban and rural folk were drawn into queue-cutting campaigns and many forms of new political celebrations (such as the five-color flag of the early republic). While the solar calendar, for example, was slowly accepted, however, it and all it stood for (the modern state, Westernization, and ultimately the Guomindang's legitimacy) never entirely replaced the traditional lunar calendar (New Year's, Qingming, Dragon-boat, and so forth).

This is a paradox that, although pointed out by Harrison herself, still seems to contradict at least that part of her argument that exclusively claims "people of China today are Chinese not because of their participation in the traditional rituals of birth, death and marriage, but because of their participation in the modern rituals of statehood" (p. 243). Indeed, the book would have benefited from clearer definitions of such concepts as identity and especially "citizenship," which Harrison sometimes treats broadly as the result of any kind of participation in any kind of state-sponsored activity and other times narrowly implies

only results from real empowerment. In particular, she seems to conflate modernity and citizenship in ways that, being left implicit, are confusing. Did a man wear a felt hat to mark himself as modern? As a citizen? As both, or possibly neither? Could one be a modern citizen in old gowns or bound feet? This reader was not entirely convinced that a new national identity was "defined precisely by the adherence to practices and customs that had previously defined what was not Chinese" (p. 24), although of course queues and bound feet did eventually disappear. If citizenship and modernity were conflated at this historical juncture, we need to know more about their precise relationship.

The book also suffers from a lack of explicit analysis of the social role of symbols and rituals. Harrison is content to tell us that, once safely dead, Sun Yat-sen became a unifying symbol (though one without political meaning). That this occurred before his deliberate apotheosis at the hand of the Nationalist government, and that Sun became a useful advertising gimmick (from cigarettes to olives, consuming the right brands proved your patriotism), is interesting but tells little else about how such symbols contributed to the new political culture, consensus, or legitimacy, if indeed they did.

Nonetheless, making excellent use of contemporary newspapers, memoirs, and also government documents, Harrison reveals the full panoply of new state rituals and fashion change across the decades. Indeed, Harrison's achievement is marked by the new questions she raises as much as by her findings, important though they are. She convincingly debunks the exaggerated gaps between elites and populace that previous scholars have posited. Harrison's research shows how the evolution of a new political culture in the first few decades of the twentieth century emerged out of a society that was generally conceived as constituted by professional sectors (bankers, students, the military, and so forth). The new political society of citizens, if not the larger "society" as a whole, marked a new ingroup that created "traditional China" precisely to serve as its "Other." This was perhaps never clearer than in the case of the Guomindang, whose rise to power under Chiang Kai-shek was marked by ceremonies and symbols that were highly restrictive, although theoretically meant to become all-inclusive in time.

The book's clear writing and illustrations make it suitable for undergraduates, while the information and argument are required reading for any serious student of modern China. This short review cannot do justice to the fascinating detail and stimulating ideas that mark every chapter. Political processes and the negotiations among various social groups created a new Chinese culture. Harrison provides us not with another reprise of student demonstrators or politicians' speeches but with peasants in lantern festivals, calculated orderings of funeral processions, and decisions about flags and national anthems.

PETER ZARROW
Academia Sinica

SHU-MEI SHIH. *The Lure of the Modern: Writing Modernism in Semicolonial China, 1917–1937*. (Berkeley Series in Interdisciplinary Studies in China, number 1.) Berkeley and Los Angeles: University of California Press. 2001. Pp. xiii, 427. \$24.95.

Literary critics and historians in and outside China have published numerous works on literature and writers of Republican China. Using many of these works as secondary sources and extensively examining primary sources in Chinese, Japanese, and English, Shu-mei Shih presents the first comprehensive study of the development of Chinese literary modernism. More than a commendable effort to construct a history of a literary movement that has long been dismissed by Eurocentricism, Sinological nativism, and the political nativism of the Chinese Communist Party, the author also problematizes Chinese modernism while investigating it as an important site of Chinese intellectuals' negotiation with Western knowledge in their pursuit of modernity. Shih locates this investigation in the changing cultural and political terrain of Republican China from 1917 to 1937 and offers a persuasive and illuminating historical account of the complex cultural production in the context of semicolonialism.

Analyzing a wide range of source materials (from short stories, novels, poems, essays, correspondence, and memoirs to interviews) produced by a large cohort of republican writers, Shih identifies three modes of negotiation with the West by May Fourth occidentalism (defined as drawing from the West for local purpose and as asymmetrical cosmopolitanism), the Beijing school (committed cosmopolitans who argued for cultural universalism), and the Shanghai new sensationism (writers who tried to maintain their aesthetic pursuit against the political pressure from the left, and who avoided representing the colonial reality by focusing on "universal" urban issues of capitalist modernity). The three-part book is organized around the development of the three processes with cogent analyses of each group's major writers' lives and works. At this level, the book also presents a genealogy of Chinese modernists from the 1910s to the 1930s that highlights well-known literary figures like Lu Xun and Guo Moruo, as well as lesser-known women writers like Lin Huiyin and Lin Shuhua.

Shih's analyses of May Fourth occidentalism, the Beijing school, and new sensationism are grounded not only in historical documents but also in theoretical engagement. Or rather, this contextualized historical study enables the author to examine issues such as orientalism, occidentalism, psychoanalysis, cosmopolitanism, mediated modernisms, colonialism, semicolonial subjectivity and agency, and so on, at both local and global levels, and to theorize from a comparative perspective. Most notably, demonstrating differences between colonialism as practiced in India and semicolonialism in Republican China, Shih proposes a framework for understanding the semicolonial subject position: a two-layered structure of strategies of

displacement through bifurcation (p. 304). She argues that the modernists who flaunted a cosmopolitan stance adopted the practice of bifurcating the "colonial West/Japan" and the "metropolitan West/Japan," and bifurcating Shanghai as a colonial city and as a capitalist city in their cultural imaginary. "By this strategy, the cosmopolitans chose not to engage with the racial, economic, and other hierarchies in their lived experience of foreign settlements, but instead chose to project their vision to faraway metropolitan centers where a liberal hegemony operated in the garb of civilization, persuasion, rule of law, and consent" (p. 374). Shih's critique of the Chinese modernists of semicolonial China is highly illuminating and relevant to contemporary China where intellectual mainstream eagerly joins the process of capitalist globalization in their pursuit of modernity.

Shih's consistent attention to unequal power relations includes gender relations. She provides excellent gender analyses of several literary texts and discerns a significant change in male representation: from the trope of the new woman in May Fourth writing to the trope of the modern girl in the new sensationism. Unlike the projection of the new woman by a generation of male writers who themselves could not overcome social and cultural constraints, the desire for the modern girl, Shih points out, "is a desire for capitalist modernity with all its seductive allures of commodification, consumption, and entertainment" (p. 301). This analysis, again, sheds light on the contemporary cultural production in China where "modern femininity" is over represented in the mass media.

In this well-researched study of cultural formation of Republican China, however, any discussion of feminism as a viable gender discourse of the time is conspicuously missing. Her discussions of female writers before the Sino-Japanese War fail to trace connections between the emergence of a cohort of women writers and May Fourth feminism. And the new woman is treated as merely a literary trope by male writers without social and historical implications. The author mentions "the rise of feminist consciousness" during the war years only in the appendix. Apparently, Shih is unaware of the close relationship between the May Fourth enlightenment project and the ascendance of feminism in modern China's political discourses. Recognition of this relationship, however, would further complicate Shih's examination of Chinese modernism.

Still, replete with astute observations of contradictions and conflicts in Chinese modernists' pursuit of modernity, Shih's book will greatly enrich readers' understanding of cultural production in China's semicolonial past.

WANG ZHENG
University of Michigan

XIN ZHANG. *Social Transformation in Modern China: The State and Local Elites in Henan, 1900-1937*. (Cam-

bridge Modern China Series.) New York: Cambridge University Press. 2000. Pp. xvi, 320. \$54.95.

During the last thirty-five years or so, there has been among China historians a growing attention to local history. Beginning with Frederic E. Wakeman's classic, *Strangers at the Gate: Social Disorder in South China, 1839-1861* (1966), and based on rich materials usually compiled by Chinese elites with the sponsorship of local officials, much of the best work done by scholars of China has focused on individual provinces, regions, counties, towns, and villages. So productive have these local studies been, especially in deepening our understanding of China's immense diversity, that most of us think at least twice before venturing any general statement about the country as a whole. Fortunately, the accumulation of excellent local studies has also prompted some venturesome historians (such as R. Bin Wong in his recent book, *China Transformed: Historical Change and the Limits of European Experience* [1997]) to forge boldly ahead and tackle not merely China alone but to reach for an even wider comparative perspective, and with outstanding results.

The book under review here does not aim quite as high as Wong did, but in his own way Xin Zhang has bravely sought to cross the local/national barrier and vault beyond it toward international comparisons concerning modernity. Based on an in-depth analysis of two regions in one Chinese province, Zhang argues that "modernity" in China must be understood differently from modernity elsewhere. China, he contends in an argument reminiscent of Wong's, has too often been analyzed according to concepts and standards devised to explain Western history. He proposes to treat in China's own terms the country's move into modernity, which he believes was effectively achieved between 1900 and 1937. He draws on Western social science where he finds it applicable but seeks to root himself firmly in Chinese realities and to shape his conceptual framework to suit those realities as he understands them. He offers what he calls an "operational" definition of modernity, in which "the facts do and are allowed to speak for themselves . . . I argue that the crucial distinction between Chinese modernity and that of the West lies in the fact Chinese modernity was based neither on industrialization nor on capitalist development" (p. 18).

The concept that emerges in the book leans most heavily on familiar ideas of political development that stress institutionalization, popular participation, and legitimization; in short, state-society relations in the process of state making. Here state making involves bureaucratic and fiscal expansion but it is above all a centuries-long struggle between the state and "various social forces competing against the state for dominance over society" (p. 217). Society, in this treatment, is essentially the arena occupied by local male power wielders and power seekers. The wider society, with its problems of gender, class, livelihood, ethnicity, religion, migration, and disease, enters marginally if at all

into this framework, suggesting to this reader that the title's reference to "social transformation" is not well chosen.

Given his self-imposed limitations, Zhang has much to offer. The north-central province of Henan was of major strategic importance. As Odoric Y. K. Wou has superbly demonstrated in *Mobilizing the Masses: Building Revolution in Henan* (1994), the province was a major seedbed for the communist revolution, and Zhang's book goes far toward explaining how that soil was fertilized between 1900 and 1937. His major theme is the displacement and/or transformation of pre-1900 elites and the emergence of new leaders and institutions after 1900. The richest and most fascinating parts of the book introduce us to relative unknowns such as Bie Tingfang. Although Bie (much to his astonishment) would be lavishly honored by Chiang Kai-shek during a personal meeting in 1938 at Chiang's invitation, which Bie at first hesitated to accept for fear that he would be arrested or killed, we learn that he was a conniving brute who rose to local power in a sea of bloodshed that included the murder of his own in-laws, a sworn brother, and a nephew whom he threw into a raging fire in order to save himself. Later he had his opium-addicted son-in-law executed as an object lesson.

Profiles of local leaders who gained power in the turmoil of the 1910s and 1920s give new and vivid meaning to the notion of a post-1911 China in both revolution and disintegration. So do the efforts at institution building, especially the mixed purposes and results of local self-government projects. Here are some very serious men, from rogues and worse such as Bie to principled patriots and a vast range in between. Zhang gives us outstanding local political history, thickly textured, viewed at ground level, and with a solid ring of truth. He has zeroed in on two very different regions of the province, one much more densely populated than the other, more urban, and closer to major national events. We see some of the inner workings of local politics in counties that here for the first time have the spotlight of modern scholarship cast upon them. We recognize the boundless ambition of most elites, whether the ruffians of southwest Henan or the smoother operators who became associates of Yuan Shikai in the more urban northern part of the province. We can detect new and constructive steps being taken in the midst of huge disorder and breakdown; even in two such different regions, certain similar patterns are visible. In particular, Zhang argues that a form of political development unique to China was able to take place in a rural peripheral area, relatively remote from urban cores, that had much in common with what was happening elsewhere in China.

Much of the argument here is convincing, and the research is powerfully impressive. The book goes a bit far in trying to make sense out of the careening nation-in-the-making that China was in those tumultuous years of revolution, civil war, cultural transformation, and foreign threat and domination. It remains

difficult to accept that China had reached something that Zhang does not too clearly identify as "modernity" by 1937. But the author promises another volume that will pick up where this one leaves off, and he may well clarify the issue further. As of now, we have in this rich book a gallant effort that overreaches but gives us robust food for thought.

MICHAEL GASSTER
Rutgers University

JEFFREY P. MASS. *Yoritomo and the Founding of the First Bakufu: The Origins of Dual Government in Japan*. Stanford: Stanford University Press. 1999. Pp. xiii, 278. \$49.50.

Jeffrey P. Mass spent his career as a historian of medieval Japan challenging received scholarly wisdom. Thus it seems fitting that, in this final work in his specialty prior to his untimely death, the wisdom that he questioned should have been his own. The book is a reworking of his first monograph, *Warrior Government in Early Medieval Japan* (1974). The new title exemplifies important changes in Mass's views over the last twenty-five years. No longer is the late twelfth century, the age of the military leader Minamoto Yoritomo, thought to have marked the eclipse of the civilian aristocracy as provincial warriors rose to dominate the court. The term "medieval," moreover, with its links to warrior hegemony in Western historiography, comes under attack as well. The result is a reassessment not only of Yoritomo's accomplishments but also of the extent to which his Kamakura period (1185–1333) belongs to the medieval rather than to the late classical age.

After his victory over the Taira (as well as his own relatives) in the Genpei civil war that ended in 1185, Yoritomo established a base in the town of Kamakura, geographically far removed from the civilian court in Kyoto. There he established a control structure called the *bakufu*, which held jurisdiction over his own men and exercised considerable influence on the court. As pictured by mid-thirteenth-century chroniclers in the *Azuma kagami*, Yoritomo organized his followers into two administrative networks: the *jitō*, who managed estates held by civilian aristocratic proprietors, and the *shugo*, military governors of provinces who served alongside civilian counterparts.

In contrast to the *Azuma kagami*'s picture of a full-blown warrior government purposefully created by Yoritomo, Mass demonstrates that the *bakufu* structure was formed through a sometimes halting, trial-and-error process, in which at least one major element—the *shugo* system—developed after Yoritomo's death rather than during his lifetime. According to the book's central thesis, Yoritomo worked within the existing system and in fact upheld it, rather than rendering ineffective such civilian organs as the throne, the retired sovereign's office, and the court. Thus *bakufu* judicial organs often supported the land rights of aristocratic proprietors against the claims of

Yoritomo's own men. Moreover, while the placement of *jitō* on estates gave Yoritomo a basis for permanent national authority (p. 254), *jitō* were "stuck" at the managerial level, unable to displace the proprietors who held ultimate rights over the land. Court and *bakufu* ruled jointly in what is termed a "dual polity." Mass demonstrates, nonetheless, that in placing aspects of realm-wide government in the hands of provincial elites from the military class, Yoritomo and his successors in the Kamakura *bakufu* set the stage for the eventual end of court domination.

While the book focuses on Yoritomo's career, two background chapters examine the rival Taira and earlier Minamoto, and Yoritomo's legacy is also examined. As in his other work, Mass bases his arguments on meticulous analysis of primary sources. Often the result is an intriguing case study, such as that of a *jitō* who encroached on the customs of the estate that he managed (pp. 231–34). Mass reviews the *bakufu* judgment of this case in detail, providing a fascinating glimpse of the *jitō*'s relations with the proprietor, on the one hand, and with estate cultivators on the other. In other discussions, the absence of names and terms in reliably dated documents leads him to question such notions as the existence of a bloc of Minamoto followers (*gokenin*) prior to the Genpei war, or Yoritomo's establishment of the *shugo* system.

If this book is not entirely ground breaking, that is because many of its arguments have been under development over the past two decades, by Mass himself as well as by some of his students. The central argument—the resilience of the courtier polity—has been featured in several other works and has become the mainstream interpretation of Kamakura-period politics. Mass takes that argument and applies it to the story of Yoritomo's career, supplying a realistic picture of his accomplishments as well as a subtle and nuanced picture of historical change.

The densely packed detail and sophisticated handling of primary sources will make this book useful to specialists in medieval Japan. Its clear writing, careful explanation of terms, and avoidance of jargon will also recommend it to nonspecialists, including advanced undergraduates.

JANET R. GOODWIN
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JAMES L. HUFFMAN. *Creating a Public: People and Press in Meiji Japan*. Honolulu: University of Hawai'i Press. 1997. Pp. x, 573. \$54.00.

This monograph seeks to explain the creation of a public in the Japanese polity between 1860 and 1920 through a careful study of the birth and development of Japanese newspapers and popular journalism. James L. Huffman shows how this public was created, what precisely the role of a Western-style free and commercial press was in it, and what the consequences have been. Focusing on the major newspapers in Tokyo and Osaka (several of which had important

national distribution), Huffman draws on both an extensive body of primary sources and the considerable Japanese-language scholarship on the topic. The result is a model monograph: meticulous research, clear exposition, and a point that matters.

Huffman's key point is that the public that was, indeed, created over fifty years was, by 1912 (the end of the Meiji period), a dual or dialectical creation of newspaper editors and the urban middle and working-class readers their papers came to serve. The mechanism connecting these two social agents was the market; these were by the 1890s commercial papers with investors to pay. Given the authoritarian tradition of Tokugawa Japan and frequent repression by the Meiji government of this new public forum, it is all the more amazing that Japan successfully developed a reasonably free commercial press. However, by a careful analysis of editors' commercial interests, government officials' paranoia, journalists' crusading, and ordinary readers' desire for news and entertainment (over opinion), Huffman shows us how this modern press contributed to the "dark valley" of popular nationalism and imperialism that stains Japan's twentieth-century history. Indeed, one of the virtues of Huffman's study is his sobering demonstration that much of the popular nationalism that drove Japan toward illiberal ends sprang precisely from the operation of press freedoms and market responsiveness of newspapers that most Westerners hold dear. A free and vibrant press, this study shows, is a necessary but not a sufficient condition for a liberal and equitable polity. Give this book to any colleague who still believes in the magic bullet of a free press.

There are several valuable insights along the way worthy of consideration by historians of Japan and comparativists. The early Japanese press in the 1850s and 1860s drew on a combination of Confucian noblesse oblige and imported Western models and technology. Huffman documents the old adage that Western tools (from cylinder presses to photography to telegraph) do not Western liberals make. The heart of the study shows how different social groups, from editors, to journalists, to government officials, to the emerging "circulation base" of readers, used these tools. The early Japanese press was divided between the *oshimbun*, or toney press of political editorials aimed at intellectuals and government leaders, and the *koshimbun*, or scandal sheets that sought sales among the urban working population. These two styles combined, under the pressures of market forces and the institutions of constitutional government, in the 1890s to produce a news-oriented commercial press that we consider modern. One key aspect of the development of a public sphere that needs to be explored further is the connection Huffman demonstrates between the developing role of the press in a public sphere and the system of public lectures, and by the 1890s public rallies, at which newspaper journalists were prominent speakers and newspapers the stimulant (by advertising the meetings) and protector (by providing a public

record of discussions and any police repression). Finally, chapter eight, on the role of the Japanese press in the Russo-Japanese War (1894–1895), provides a troubling mirror for post-September 11, 2001 press and public activities in America.

This will be the standard work on the birth of Japanese journalism for our generation. It includes a helpful appendix on the actual newspapers, chronology, and fifty journalists. Its rich primary resource base and clear presentation make it a rewarding resource for comparative historians. Huffman smoothes the way with strategic references to similar issues in British and European press history. Historians can learn a great deal about Japan's entry into the "new world order" of the twentieth century through this lucid volume, which reveals the historical contingency of change as the press and the people of Meiji Japan created each other.

TIMOTHY CHEEK
Colorado College

MASAHIRO YAMAMOTO. *Nanking: Anatomy of an Atrocity*. Westport, Conn.: Praeger. 2000. pp. xv, 352. \$39.95.

JOSHUA A. FOGEL. *The Nanjing Massacre in History and Historiography*. (Asia: Local Studies/Global Themes, number 2.) Berkeley and Los Angeles: University of California Press. 2000. Pp. xvi, 248. Cloth \$40.00, paper \$15.95.

Few issues in the history of twentieth-century East Asia have generated as much passionate debate as the atrocities committed by Japanese troops against Chinese prisoners of war and civilians during the capture of Nanjing in 1937–1938. The publication of Iris Chang's *The Rape of Nanking: The Forgotten Holocaust of World War II* (1997) has brought both the historical event and the historiographic debate to the attention of a broad audience. Two recent studies contribute much-needed scholarly perspectives to English-language treatments of this subject and help to explain the persistence of conflicting historical narratives of the Nanjing massacre.

Masahiro Yamamoto presents an analysis of Japanese atrocities from the standpoint of a self-described "centrist-revisionist" who affirms that Japanese troops did, in fact, engage in large-scale atrocities during the capture and occupation of the city but questions the magnitude and scope of killing alleged in most Western or Chinese accounts. Using Japanese army documents and the diaries of soldiers involved, in addition to more frequently used Chinese and Western sources, he estimates that as many as 50,000 Chinese were "killed in unlawful ways" (p. 115). He finds that all but 7,000 of the victims were prisoners of war, soldiers out of uniform, and conscription-age civilian men suspected of being former soldiers, who were rounded up and systematically executed by army units on verbal orders from the staff of the Shanghai Expeditionary Army. At the same time, he argues that credible

evidence for larger numbers of victims cited in the verdicts of the Tokyo War Crimes trials and in postwar Chinese and Western accounts is lacking. He attributes the steady inflation of numbers in the aftermath of the event to wartime propaganda and to postwar politics and diplomacy. Yamamoto's affirmation of large-scale mass killings pits him against the hardline "revisionists" like Masaaki Tanaka, who deny that any significant atrocities occurred. At the same time, his analysis places him at odds with "traditionalist" Japanese scholars, as well as a majority of Chinese and Western writers, who argue for a far larger scale of slaughter that took the lives of between 100,000 and 400,000 defenseless victims.

Although Yamamoto's estimates place him in a position between the "traditionalist" and "revisionist" poles of the debate, his "revisionist" leanings become evident in his efforts to explain the causes of the atrocity. Poor planning, including the lack of provisions for securing large numbers of prisoners; the discovery of some handguns and grenades in the possession of surrendered troops; and the fear of "plainclothes soldiers" engaging in guerrilla operations all contributed to the practice of mass execution as a means of eliminating threats to the security of the occupation forces. Yamamoto attempts to provide a broader explanatory framework by including a chapter on the history of military atrocities, noting that Japanese actions in Nanjing had numerous precedents. He argues that although Japanese atrocities were not excusable, they were, nonetheless, dictated by military purposes and not by some atavistic blood lust or behavioral imperatives inherent in Japanese culture. Yamamoto takes care to reject "relativist" arguments that regard the Japanese army's actions as no worse than what other armies have done elsewhere, yet his emphasis on precedents in the history of warfare reflects an underlying apologist tone that informs much of the book. For this reason, English-language readers may find this study more "revisionist" than "centrist" in orientation and its contribution to exposing the reality of at least some of the mass killings overshadowed by its efforts at mitigating explanation.

The collection of essays edited by Joshua A. Fogel engages the discourse over the Nanjing massacre in a very different way. The contributors all agree that the Japanese army carried out atrocities of monstrous proportions in Nanjing, but their primary concern lies in analyzing the conflicting trends in historiography rather than in joining the fray directly. Mark Eykholt explores the evolution of the memory and meaning of the events in Nanjing to the Chinese people and how the massacre became a powerful symbol of the modern Chinese experience, unifying a broad spectrum of otherwise disparate groups in the mainland, Taiwan, Hong Kong, and overseas Chinese communities. For English-language readers, Eykholt provides valuable background for a critical appreciation of the passion that Chinese activists and intellectuals bring to the debate as well as an understanding of factors placing

limitations on more open historical inquiry in China. Takashi Yoshida introduces the long-standing "battle over history" in Japan between "progressives," who affirm the reality of the atrocities and acknowledge Japanese responsibility, and "revisionists," who either downplay the scale of the killings, or in the extreme case, deny them outright. He traces the evolution of competing positions from 1945 through the 1990s and examines the forces shaping the debate, including its connection to the broader politics of war memory in postwar Japan. Significantly, he notes that some Japanese revisionists approve of the Smithsonian's treatment of the atomic bombing of Japan in the Enola Gay exhibit in 1995 as reflective of a "healthy nationalism" in America and regard their own attitude toward war crimes allegedly committed by the Japanese military as comparable. Given that awareness of such heated discussions remains limited outside of Japan, Yoshida's essay provides valuable insight into the complexity of Japanese perspectives and a corrective to the notion of a monolithic Japanese view.

Daqing Yang offers what is perhaps the most ambitious of the three essays in the collection and delves into the problem of historical epistemology. The distinction between dispassionate or objective historical writing, as opposed to myth or polemic, might be made theoretically, he argues, but it becomes a problematic conceit in practice. Ignoring the moral and political context of historical inquiry creates obstacles to meaningful dialog among those engaged in the production of divergent narratives. Self-awareness on the part of academic historians of their role in the creation of knowledge is essential, and, rather than standing aloof from the clash of competing narratives, they must apply the tools of their craft to an active engagement in the competition. Yang's essay highlights the broader significance of the undertaking represented by this book. In its own way, each of the contributions explores the nature of historical knowledge, the political uses of history, and the evolution of historical memory. An understanding of these issues is indispensable to examining a cataclysmic event such as the Nanjing massacre.

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TIM MAGA. *Judgment at Tokyo: The Japanese War Crimes Trials*. Lexington: University Press of Kentucky. 2001. Pp. xiv, 181. \$25.00.

This book consists of a preface; five chapters and epilogue (151 pages); notes, bibliography, and index. Chapters two, three, and five deal with the International Military Tribunal for the Far East (the IMTFE) and its aftermath; chapter one with two lesser trials; and chapter four with trials on Guam. Tim Maga's "general thesis" is that there "might have been good intentions behind the Tokyo trials and that they might even have done good work" (p. ix), but he contradicts himself by concluding, crudely, that the trials were

"flawed as hell" (p. 138). I like slim books, but this one never should have seen the light of day. I say so not because I disagree with its rambling and self-contradictory argument (I do disagree) but because the scholarship is shallow, shoddy, and irresponsible.

With one insignificant exception, the author uses only English-language sources on a subject on which there is much Japanese-language scholarship. Moreover, his bibliography ignores important English-language sources, including the basic trial documents: *The Tokyo War Crimes Trial: The Complete Transcripts* (1981); *The Tokyo War Crimes Trial: Index and Guide* (5 vols., 1981–1987); and *The Tokyo Major War Crimes Trial: The Records* (124 vols., 1998–). Maga is captive to his sources: administrative archives, not transcripts or scholarly criticism. What point is served in restating, half a century later, prosecution claims?

Maga conflates the IMTFE (or Tokyo trial, singular) and other postwar trials of accused Japanese: one in Tokyo (Tsuchiya), one in Manila (Yamashita), and the Navy trials on Guam. What is their connection to Tokyo? The Tokyo trial was international in form, placed Japan's wartime leaders in the dock, and charged crimes against peace and against humanity. The other trials were purely American, dealt with smaller fry, and stuck largely to conventional war crimes. Conclusions about them have no necessary impact on judgments about Tokyo.

The author demonstrates a weak grasp of legal issues. He lists fifteen diverse titles, from participant accounts to very dubious scholarship, and states that "This select bibliography represents an analytical framework." He offers little extended legal or procedural analysis.

Maga makes factual errors too numerous to list here, but they include citing the Tokyo trial in the wrong city (Yokohama, p. 2). The author also cites at length (pp. 4–5, 52) one Hiro Nishikawa, "historian and Japanese legal expert." His sources are two lectures, apparently in English to American audiences, and personal communications. Despite his prominence in chapter one, Nishikawa apparently has not published on this topic.

Maga misuses footnotes and index. Consider note 6 (p. 158): "For 'deep background' [*sic*] on Keenan's work and views . . . contact James Zobel, archivist and Tokyo War Crimes Trials specialist, at the MacArthur Memorial (telephone, 757–441–2965)." Or note 10 (p. 153): "Various notes and handout material from this spring 1995 seminar [at Harvard] are available from the author." The index governs only one content note, does not list Nishikawa, but does list the author's wife and a friend, both of whom appear only for thanks in his preface.

Maga attributes to me (p. 7) words and arguments that appear nowhere in my book, *Victors' Justice: The Tokyo War Crimes Trial* (1971). He summons Michael Walzer's *Just and Unjust Wars* (1977) in praise of Tokyo's chief prosecutor Joseph Keenan: "Yet in the opinion of . . . Walzer, Keenan's approach was never

misplaced" (p. 121). Walzer's book nowhere mentions Tokyo, let alone Keenan; the passage is a general conclusion that it is better not to let war crimes go by the board.

In a version of his preface that appeared in the *Chronicle of Higher Education* (May 4, 2001), the author states that he is up against "politically correct academics" and that he has been warned not to publish this book "for the sake of my career." Will this book harm his career? Yes—not because it is politically incorrect but because it is such inferior work.

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INES G. ŽUPANOV. *Disputed Mission: Jesuit Experiments and Brahmanical Knowledge in Seventeenth-century India*. New York: Oxford University Press. 1999. Pp. xiii, 277. \$23.95.

This book is as much about literary devices, languages, and semiotics as it is about the realities of the Jesuits and Jesuit missions in India in the seventeenth century. What saves it from being a barren abstract analysis of "text" is that the author does provide an account of the context and, among other things, is concerned with the external conditions and developments that influenced the Jesuits and Jesuit writing. Indeed, one of the main values of the work is that Ines G. Županov pays very careful attention to European as well as to Indian history and, as a result, is able to show how the European heritage (including the conventions of Jesuit writing) affected their accounts of Indian religion, mission, and society.

The author selects four basic "tropological and narrative frameworks" that become the basic sections of the book. These are the "Dialogic/Polemical Mode," which was the framework in which Roberto de Nobili and the Jesuits debated the issue of the Malabar rites; the "Geo-ethnographic Mode," which contrasts "Aristocratic Analogies" with "Demotic Descriptions"; the "Theatrical Mode," where the author discusses conversion scenarios, miracles, and other encounters; and the "Self-expressive Mode," in which Nobili in particular communicated his dreams of Utopia and aspirations to saintliness in the context of the Madurai mission.

One of the clear implications of this work is that literary techniques and conventions shaping and conditioning texts in one period can lead to misunderstandings when read by scholars immersed in quite different worlds of literary convention in another. Furthermore, by grappling with the meaning of the text, Županov does succeed in raising anew further questions about the Jesuits and the nature and impact of the Madurai mission.

Admittedly much of what Županov has to say about the role of politics in the controversy over the Malabar rites, the impact of national prejudice, and the strength of individual agendas is not especially new—except

perhaps for some of the detail. What is more significant is the author's emphasis on conflicting European ways of viewing and describing the world outside of Europe. Županov contrasts Nobili's view with that of his arch rival, the somewhat pedestrian Jesuit, Gonçalo Fernandes, who was already entrenched at Madurai when Nobili arrived in 1606. While Fernandes focused on externals and saw difference, Nobili went further, through interpretation, finding parallels in the thinking and theology behind different rites and practices. Furthermore, Nobili's universalistic outlook, reflected in his use of similes in language, was reinforced by his belief that the Brahmans were descended from a lost Jewish tribe and that Hindu theological texts were "defective Catholicism" (p. 115).

Nevertheless, Županov sees one important similarity in the outlook of Nobili and Fernandes: their belief in the overriding importance of "Brahmanism." Both men privileged the "text" and regarded Brahmans as the key group for understanding Indian religion and society. Although this is one of the central points in the book, the author gives no completely satisfactory explanation of why the Jesuits adopted this view. Of course Nobili and other Jesuits used Brahman pundits and informants, but were these informants the only ones? As the book makes clear, the Jesuits eventually introduced a new kind of "nativized" missionary model—the *pañtārācāmī*—which, it was hoped, would co-exist with the earlier model of the Brahman *sannyasi*, and which would enable them to reach the lower echelons of the population. What was the extent of Jesuit contact with the lower levels of the population? And what did they really know about untouchable and other low-caste practices and views of the world? If the Jesuits did have contact with all the major classes in Hindu society, why adopt the Brahman model? Furthermore, how does one explain the extraordinary fact that Fernandes, pastor for ten or more years to the low-caste Parava Christians, also espoused the idea that the Brahmans were the key to an understanding of Indian religion and society? Is this what the Paravas told him? If not, why did he believe in the Brahmanic model? What difference did new knowledge or information actually make, and are there times when a paradigm is so powerful that it overrides all the messages of common sense and experience?

Furthermore, while there can be no doubt that the Jesuits developed a "Brahmanical" or "top-down" theory of Indian civilization, it is quite another thing for the author to argue that it was the Jesuits alone who created the model or made it "axiomatic in the centuries to follow" (p. 27). Even before the Madurai mission, travelers like Ludivico di Varthema were pointing to Brahmans as "the chief persons of the faith" in India. Furthermore, we cannot assume that the Jesuits were entirely responsible for Protestants adopting much the same idea. There can be little doubt that the activity and publications of the Bengal Asiatic Society influenced Baptist and other Protestant views in favor of the Brahmanical model. But even if

the Jesuits had some influence on the Asiatic Society's work, the direct influence of Brahman pundits was also present in the Hastings administration as well as in scholarly circles. Hence, this independent continuing brahmanical influence has also to be taken into account. It might therefore be more appropriate to think of the Jesuits as one influence among a number of factors encouraging the formation of the European paradigm.

If the extent to which this book adds to our existing knowledge and understanding of the Jesuit mission is somewhat problematic, it certainly does raise important questions that require further investigation.

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SHAHARYAR M. KHAN. *The Begums of Bhopal: A Dynasty of Women Rulers in Raj India*. London and New York: I. B. Tauris. 2000. Pp. x, 276. \$39.50.

This is a family history written by the son of Abida Sultaan, the last woman to be named heir to the throne of Bhopal. Hamidullah, her father and the last nawab, died after Bhopal had become part of India, and by then Abida had emigrated to Pakistan. This history begins with Dost Mohammed Khan, the founder of Bhopal, and traces the lives of Mamola Bai, wife of Dost's heir and regent for her two stepsons Faiz and Hayat, and Bhopal's four women rulers: Qudsia Begum, 1819–1837; Sikandar Begum, 1844–1868; Shahjehan Begum, 1868–1901; and Sultan Jahan Begum, 1901–1926. The book is arranged chronologically with chapters devoted to these key individuals. Exceptions are chapters on the siege of Bhopal, 1812–1813, and Sultan Jahan's successful efforts to have the British recognize her son Hamid as heir, and a concluding chapter that assesses the rule of the begums. An epilogue completes the history of Bhopal and of the family to the present.

In the preface, Shaharyar M. Khan states that he is not a historian but a career diplomat who began this project with a memoir written by his mother and decided to write a family history. He has mined autobiographies, biographies, letters, correspondence with the British, and family lore for details of court struggles, intrigues and alliances, relationships, and personal traits.

The author documents the training and education of the four begums who took control of Bhopal and their tenure as rulers. That they were Muslim women, presiding over an overwhelmingly Hindu state during a tumultuous time in Indian history, makes this a remarkable story. The book is replete with details of them fighting for the right, and the right of their heirs, to rule; throwing off, reassuming, and again throwing off purdah; leading armies and hunting with men; and ably administering Bhopal. According to Khan, the explanation of why this happened lies in the personalities of the women, absence of male heirs, dynastic politics, and the British presence. Unfortunately, he

ascribes their ability to rule successfully to "a woman's instinct to hold and preserve what she owns" (p. 216).

This book has some flaws that could well have been corrected by a careful copyeditor. First, there are a number of contradictions. For example, writing about the feud between Shahjehan and her daughter Sultan Jahan, Khan says Sultan Jahan removed her daughter Bilqis from the grandmother's care when Bilqis was ill. Later, when the child was dying, Sultan Jahan begged her mother to come and see Bilqis. But in the final chapter the author blames Sultan Jahan for not allowing Bilqis to see her beloved grandmother. Second, errors in chronology undermine the book's value for historians. The author places Sultan Jahan's speech to the All India Women's Conference (AIWC) in Delhi in 1912 even though the AIWC did not hold its first meeting in Poona until January of 1927. Third, the book is marred by unfortunate metaphors and allusions. The begums are often referred to as "Amazons" (p. 86); Sikander was like Annie Oakley (p. 79); and Shahjehan was "Trilby to his [her husband Siddiq Hassan's] Svengali" (p. 122).

The work that will place the begums of Bhopal in historiographical context has yet to be written. Claudia Preckel's *Begums of Bhopal* (2000) is also a chronological narration of Bhopal under the four begums, but it is based on fewer sources and contains less detail than Khan's book. Feminist scholars Uma Chakravarti and Kumkum Roy explain the neglect of powerful women in Indian history, such as the begums of Bhopal, as a consequence of historians' allegiance to patriarchy and the state (see Chakravarti and Roy, "In Search of Our Past," *Economic and Political Weekly* XXII, No. 18 [April 30, 1988]).

The begums of Bhopal were truly extraordinary women, and Khan's account, with all its flaws, goes a long way toward giving us a picture of their reign. The author is at his best when describing the day-to-day lives of the last three begums. His accounts of the pilgrimages to Mecca made by Sikander and Sultan Jahan are delightful. I was especially interested in the different begums' decisions to unveil, veil, remove the veil for dramatic effect, and, finally, promote unveiling as reform. Also fascinating were accounts of how the begums negotiated Islamic tenets. All in all, this book is a good read and will be of interest to historians interested in the princely states and prominent women in Indian history.

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ELSBETH LOCHER-SCHOLTEN. *Women and the Colonial State: Essays on Gender and Modernity in the Netherlands Indies 1900–1942*. Amsterdam: Amsterdam University Press; distributed by University of Michigan Press, Ann Arbor. 2000. Pp. 251. \$27.50.

With these six essays exploring women's lives in the colonial Dutch East Indies, Elsbeth Locher-Scholten

makes an important contribution to Indonesian history. Focusing on Java, where the majority of the population lives, the essays address the situation of both European and indigenous women and, in the process, reveal the racial and class divisions that kept them apart. The colonial state, in particular its half-hearted gestures toward democracy, appears here as a significant background against which women's lives unfolded. Locher-Scholten's materials come partly from the archival spoors of that state (its Labor Office and the People's Council or *Volksraad*, for example) but also from household manuals aimed at Dutch women on their way to the tropics, and from advertisements and children's literature depicting life in the colony. The glimpses of life in the Indies thus include how women appeared in legal discourse and public media, but also how race, gender, and class shaped even the most private spheres such as marriage, dress, and cooking.

Little noticed outside the People's Council where it transpired was the debate about women's night labor that took place in the mid-1920s. The "progressive" view of this matter was that the weaker sex, and the children who depended on her, merited the law's protection against her working at night. Businessmen in the sugar and tea industries that utilized such labor countered that Javanese women worked primarily because of *adat* (tradition) rather than from simple economic need, and it was best not to meddle with the dictates of tradition. On this point, Locher-Scholten examines three surveys from the 1920s and 1930s showing women's participation in agriculture, including plantation agriculture. The numbers varied considerably by locale and by the availability of other income-generating activities, but it seems fairly clear that women's agricultural labor on Java, like that of men, was motivated in the first instance by economic factors, although the necessity to work had surely become traditional, especially for poor women.

The way *adat* figured into the debate over night labor was in fact typical of other issues. During the decades-long discussion about women's suffrage, the argument about *adat* was again used at times to exclude Indonesian women from this privilege. However, Indonesian feminists were not nearly so concerned to gain voting rights as were European women in the Indies and elsewhere. Indonesian feminist organizations focused, rather, on reforming education and marriage. In any case, suffrage meant very little where the only bodies elected exclusively by vote were city councils, and where the highest representative chamber in the land, the People's Council, claimed only advisory power. Suffrage was granted to all males in the Indies in 1925, given certain property and literacy provisions. Not until 1937 did the People's Council accord women the same prerogative, by which time even right-wing organizations were on the women's suffrage bandwagon. By the time the colonial administration approved, however, it was too late: the Japa-

nese invasion of 1942 preempted women's exercise of the vote.

European women in the Indies have not been portrayed as negatively as they have been in literature about British colonialism, where they often appear more racist and exclusionary than male colonials. To investigate the Dutch case, Locher-Scholten examines the discourse about household servants, the Indonesians with whom European women had the most intense and frequent contact. How-to manuals of the time advised that a household should expect to hire at least seven people to provide for its child care, cooking, cleaning, laundry, garden, and transportation needs. This discourse about household management, no more or less racist than the discourses dominated by European men, showed a deep ambivalence toward the servants who shared one's home. Praised for loyalty and devotion, servants were also excoriated for spoiling the children in their care and were themselves reduced to metaphorical children who required much maternal patience and oversight.

As time went on, European mothers were counseled not to give over too much of the children's care to the *babu* (nursemaid), and, in this same period, firmer symbolic and practical lines were drawn separating Europeans from the Indonesians who surrounded them. Advice about not "going native" that was evident in the early twentieth century had all but disappeared by the 1920s and 1930s, when there was no longer significant danger of that. The numbers of European-born women in the Indies were burgeoning, and lifestyles there could more closely imitate those of the metropole. Through the nineteenth century, for example, European women at home in the Indies had worn the native sarong and loose-fitting *kebaya*, a sensible alternative to the petticoats and dresses they had brought to the tropics. As women in Europe began to wear shorter, sleeveless dresses, and even pants, and as women in the Indies measured themselves according to metropolitan standards, the native dress became old-fashioned. Likewise, the rice that had long been the staple of colonial diets was replaced by canned and other imported foods that set Europeans apart from their servants and Indonesian neighbors.

Framing the five substantive essays in this volume is an introductory essay wherein the author places the Dutch colonial case against a comparative background of colonial and feminist theory. There she provides also an outline of the Indonesian nationalist movement, essential background to the politics of the state depicted in her essays. The rich details in this collection are both interesting to read and carefully documented. In the end, these details speak most pointedly to the limits both of feminism ("female solidarity across the colonial divide did not exist" [p. 35]) and of democracy in the Dutch East Indies.

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BRIAN McALLISTER LINN. *The Philippine War 1899–1902*. (Modern War Studies.) Lawrence: University Press of Kansas. 2000. Pp. xiv, 427. \$39.95.

It may not be fair to compare a book written about the past with events in the present, but there are some striking congruities between the wars waged by America against the Philippines between 1899–1902 and the recent one in Central Asia. On the first occasion, education was imposed on Filipinos through force and annexation, while in the latter case Afghans were simultaneously bombarded with food parcels and heavy ordinance. Both are strange companions, and in both instances it is apparently done for their own good: it was called “benevolent assimilation” a century ago, and now it is reportedly “a strike on Afghanistan” to bring back music, kites, and women to the streets of Kabul. Given the circumstances, I would find the publication of any study, no matter the degree of scholarship, that presents only one side of the story a case of unfortunate timing. And Brian McAllister Linn’s book does just that; despite its careful protestations to the contrary and its useful insights into the war from the U.S. perspective, it reads like an apologia to excuse American actions in the Philippines without coming to terms with the central question of what their soldiers were doing there in the first place.

Linn sets out to “correct” the idea that a relatively small U.S. expeditionary force “terrorized” seven million Filipinos into submission (p. ix). He argues that it was neither a brutal war of racist soldiery slaughtering defenceless natives nor a selfless one waged by Americans to save their “little brown brothers” but “a more complex and challenging phenomenon” (p. 328). Structurally, too, he endeavors to convey the diversity of the conflict around the archipelago by dividing the book into two parts: the first dealing with the more conventional operations that took place in central Luzon between 1899 and early 1900, and the second covering the guerrilla warfare and pacification campaigns that were waged in the different regions. This is an important distinction that gives greater emphasis to the course of the war in non-Tagalog areas that have too often been obscured by a dominant nationalist historiography even within the Philippines. Some of Linn’s strongest chapters draw attention to these regional conflicts and give greater prominence to the interethnic rivalries and often open hostility to Tagalog leadership. Moreover, his assessment of Emilio Aguinaldo’s negligible influence on subsequent events after his flight over the mountains in 1900, although controversial, conforms to his archipelago-wide picture of local commanders waging their own campaigns according to circumstance and the resources at hand virtually ignoring their head of government in his retreat at Palanan.

Apart from this important insight, however, the study is very much a one-sided affair; there is only superficial treatment of the conduct of the war from the Filipino perspective and little reference to non-

American sources. Even when Linn consults Philippine historians, they are invariably also his fellow countrymen, such as Glenn A. May, Alfred W. McCoy, or Norman G. Owen. In all fairness, such recourses are frequently unavoidable, but nevertheless it does detract from his attempt to present a more balanced account of hostilities and to explode the mythology that has grown up about it. Linn’s efforts are also compounded by problems of his own making. He renames the conflict “The Philippine War,” rejecting as pejorative the use of “The Philippine Insurrection” and dismissing the term “Philippine-American War” (and its variants) on the grounds that it conveys the idea the conflict was one between nation-states. Putting aside debate over the meaning of the term, Linn’s failure to recognize the Philippine declaration of independence as a meaningful act of sovereignty already undermines the neutrality he avers. He only compounds this impression by continuing to refer to the various Filipino forces as “insurrectos,” rebels, and insurgents and berating the Army of Liberation for its poor performance in 1899 and its “inability to deliver a salutary defeat upon the inexperienced [U.S.] 8th Corp” (p. 62). Not to accord the Malolos Republic the status of a state and then to condemn it for not having a professional national army seems somewhat hypocritical. A similar contradiction can be seen in Linn’s treatment of alleged American “atrocities” that he considers are too often the product of other historians “imposing their own values on the past” (p. 221). Perhaps the author himself falls victim to a national conceit that often accords too little respect to things un-American.

Amid all the noise and bustle of war as Linn charts the movement of regiments and columns about the map of the archipelago, he remains deafeningly silent about other factors that may have significantly contributed to the victory of one side over the other. Climate and disease are too often ignored and too little studied by historians. The Philippine-American War took place during one of the strongest El Niño dry events of the twentieth century. While Linn gives graphic accounts of how the mobility of U.S. soldiers was hampered by terrain and limited by weather, there would actually have been fewer typhoons and less rainfall than in other years. Similarly, animal epidemics raged throughout the archipelago, rinderpest among carabaos and surra among horses, decimating herds and causing untold hardship to Filipinos in terms of both agricultural productivity and overland mobility. Linn barely mentions the former and completely ignores the latter, when the importance of the small native horse that provided the chief means of land transportation in the islands was enhanced by an American naval blockade that restricted seaborne communications. To what extent did these factors advantage the American cause? Were the effects of General J. Franklin Bell’s “reconcentration” policies in Batangas (incidentally a center of stockbreeding) exacerbated as drought aggravated famine and the close proximity of animals

spread disease? These are tantalizing questions that also need to be addressed to come up with a more balanced account of the war.

This is a great book if you are an American and a military history buff. It also makes a significant contribution to a better understanding of the course of the war, especially from the American perspective. There remains, however, plenty of scope for someone to give us a still more balanced and corrected view of events that are so little remembered outside of the Philippines and yet so strangely resonant in today's world.

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CANADA AND THE UNITED STATES

ROBERT LEWIS. *Manufacturing Montreal: The Making of an Industrial Landscape, 1850 to 1930*. Baltimore: Johns Hopkins University Press. 2000. Pp. xvii, 336. \$45.00.

Robert Lewis sets out to challenge the dominant interpretation of urban historiography that posits that residential and industrial centralization was mainly a middle-class phenomenon, and that working-class suburbanization was relatively unimportant before the end of the 1930s. Lewis accomplishes this objective by chronicling the historical geography of Montreal's manufacturing districts between 1890 and 1929. Lewis argues that the city's manufacturing and working-class districts were constructed as a response by business elites to the needs of manufacturers to implement new economies of scale, new technologies of production, and new labor processes in new spaces. Case studies of several of the city's important industries illuminate the dynamics underlying the locational decisions of manufacturers. Lewis uncovers the inter-firm and inter-industry connections in the formation of manufacturing districts that laid the basis for the development of both central and suburban manufacturing districts in the late nineteenth and early twentieth centuries.

The book is organized into two chronological sections. The first section covers the period from 1850 to 1890, when an industrial district physically and socially distant from the central districts emerged. Montreal's transition from a commercial city to an industrial one in the 1850s was similar to that of other North American cities. The expansion of industrial production and the emergence of a range of manufacturing pathways and uneven development characterized the industrial growth of Montreal, as Lewis's case studies of the clothing, printing, and carriage-making industries illustrates. The transition from commercial to industrial capitalism, Lewis explains, involved people from various backgrounds and the collaboration of several forms of capital, although the line between a mercantile commercial elite and an industrial bourgeoisie was never clear. The city reached its population limits in the 1880s, and thereafter suburban growth

surpassed that of the city. Lewis describes in elaborate detail the emergence of manufacturing districts on the fringe areas of the West and East Ends: first the mid-century districts of Griffintown, Canal, and Sainte-Marie, and later Hochelaga, Saint-Henri, Sainte-Cunégonde, and Côte Saint-Paul. In the second part of the book, spanning the period from 1890 to 1929, Lewis refutes traditional interpretations that a simple polarization of scale and industrial type between core and suburb occurred. Not only did large, capital-intensive firms employing the latest production methods move to the suburbs to take advantage of cheap land and technological advances in transportation, but new manufacturing districts on the urban periphery featured a range of firms and scales, ranging from small independent workshops to large corporate branch plants. Corporate consolidation and the construction of U.S. branch plants contributed to the increasing scale of production. Lewis points out that a significant number of suburban residents were also working class, thus challenging the traditional historiography in the field.

Lewis utilizes a variety of primary sources including fire insurance maps, industrial journals, newspapers, government reports, city directories, and booster pamphlets. His analysis of the development of manufacturing in Montreal relies extensively on rental data contained in the city's water tax rolls beginning in 1847, the year they were first compiled. The water tax rolls are used to determine the size of the manufacturing enterprises and employment scale.

Well researched and providing a comprehensive picture of industrializing Montreal, Lewis's analysis leaves some questions unanswered. He posits that the working class was a central presence in the transition from commercial to industrial capitalism and the expansion of suburbia, yet the working class remains an elusive presence in the background. We see little of the agency of working-class families in ethnic neighborhoods struggling to survive in industrializing Montreal, which has been so richly portrayed in studies of nineteenth-century working-class Montreal by Bettina Bradbury and Bruno Ramirez. Lewis also points to the importance of middle-class reformers and planners in shaping the development of Montreal, but he does not probe the social relations between reformers and manufacturers in any depth. The uniqueness of Montreal also needs to be uncovered. Lewis points to anglophone-francophone tensions in municipal politics but does not discuss this conflict.

These criticisms aside, this is a useful book for understanding the complexities of industrial-capitalist transformation. Used alongside the writings of social historians, we now have a fuller understanding of industrial decentralization in nineteenth and early twentieth-century Montreal.

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MICHAEL BLISS. *William Osler: A Life in Medicine*. New York: Oxford University Press. 1999. Pp. xiv, 581. \$35.00.

When Harvey Cushing donated his collection of Osleriana, he remarked that "they will help someone some day to write a better, briefer, and more accurate biography than I have done" (p. 489). Michael Bliss has done so, unearthing additional primary sources and providing a vivid, intimate portrait of a man who is widely regarded as a model physician. This masterly synthesis captures the spirit of the man, his class, and his era. By virtue of Osler's standing in the Anglo-American medical profession at a pivotal moment, it also depicts the landscape of the elite world of scientific medicine.

The highlights of Osler's half-century-long career are easily summarized. Raised in rural mid-nineteenth-century Canada, in a modest but well-connected family, he sought out the best available medical training and, by the age of twenty-five, was imparting it to McGill University students. His reputation as an almost prescient diagnostician, a compassionate clinician, an up-to-date scientist, and a prodigious worker won him a "call" to the University of Pennsylvania and, later, to the new Johns Hopkins University and Hospital. Osler literally "wrote the book" on the *Principles and Practice of Medicine* (1892). Legendary as a teacher, awesome as a bibliophile and classical scholar, and eminently successful in a fashionable practice, Osler capped his career as Regius Professor of Medicine at Oxford University.

But the roster of Osler's impressive accomplishments scarcely imparts to the uninitiated his impact on his contemporaries, his students, and later generations of physicians. Grace Revere Osler called her husband "a nearly saintly person" (p. 448). Others have been less restrained. Nonetheless, Bliss—Devil's Advocate?—resisted writing a hagiography. His meticulous research uncovered no unsavory side to the beatified physician, yet he concluded—in an appropriately Anglican gloss—that Osler was no saint but "medicine's . . . Archbishop of Canterbury" (p. 501).

Bliss's powerful writing draws us into Osler's charmed circle, sharing both his triumphs and his tragedies. Yet we expect a scholarly biographer to offer also a more critical perspective. Canonization proceedings, after all, reveal at least as much about the sanctifiers as the sanctified. Bliss helps us understand how Osler, preeminently a classifier and cataloguer of disease as well as books, was rooted in nineteenth-century scientific medicine and why he eventually feared "losing his edge." By 1900, Osler's scant interest in using his clinical laboratory for experimental work truly dated him. Bliss also draws important connections regarding "Osler's search for a medical middle way" in a wide variety of contexts: from disputes over "full-time" clinical professorships and psychotherapy to models of angina and the clinical significance of blood pressure.

Elsewhere, he is less insightful: for example, his description of Osler as "therapeutically . . . not so much a nihilist as a progressive conservative . . . [whose] most obvious guiding star was a therapy's effectiveness in easing suffering" (p. 190). His characterization of Osler's philosophy of science conflates Kuhnian paradigm shifts with the Platonic growth of truth, and his presentist description of Osler as a medical ethicist is questionable. One wonders, too, why Bliss chose to conclude with a speculation about possible future revelations from DNA analysis of the relics of Osler's brain.

More troubling is the author's somewhat defensive stance regarding Osler's social prejudices. While pervasive among men of his social class, his views on women in medicine, his (at best) insensitivity to racial discrimination, and his undisguised paternalism were by no means universal in his own time. Even without invoking an ahistorical "political correctness," a more objective assessment is surely possible. For example, Bliss discusses several episodes involving the segregation of the wards of Johns Hopkins Hospital, and casually endorses Osler's (unspecified) generalizations about disease prevalence amongst black Baltimoreans. His readers would have been better served by a more balanced presentation that included discussion of these issues in the black press, and among black physicians, as well as current scholarship.

Bliss devotes substantial attention to the apparent paradox that a man who opposed women in medicine was nonetheless revered by many female students as well as patients. Here, too, he is more defensive than impartial. Some anecdotes of Osler's "teasing"—and even of his humanitarianism, as when he arranges medical care for a sick child, while giving the impoverished mother money to get drunk—are difficult to read without cringing. And while Bliss questions the motives of the women whose funding was crucial to Johns Hopkins—noting that Martha Carey Thomas's anti-Semitism at Bryn Mawr undercut any claim to an "abstract stand for human rights"—he is not similarly critical of Osler. It is particularly disappointing that such a distinguished historian would refer casually to the "new female professionals" of the 1960s and 1970s as "insecure revolutionaries," and puzzling that he would remark, a few paragraphs later, that Jane Austen and George Orwell (but not Karl Marx or Sigmund Freud) have "persisted" to the present (p. 499).

Nonetheless, this biography is a monumental accomplishment. Osler openly avowed a duty to "knit together the generations of physicians by honoring the great men of the past in acts of 'filial piety'" (p. 249). Bliss has made an outstanding and eminently readable contribution to this endeavor, as well as to the scholarly study of the history of medicine.

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ROGAN KERSH. *Dreams of a More Perfect Union*. Ithaca: Cornell University Press. 2001. Pp. xi, 358. \$39.95.

Rogan Kersh provides an insightful history of the concept of "union" in American political and popular discourse. As even the most cursory glance at nineteenth-century politics would demonstrate, union has been among the most important and contested concepts in American politics. The concept of union does not fit neatly into the standard liberal and republican paradigms that have framed much of the scholarship on American political thought over the past few decades, however. As a consequence, union has received substantially less attention than such equally vital concepts as "liberty," "equality," "community," or "virtue."

As Kersh amply demonstrates, the scholarly oversight of union has been unfortunate. Understanding the concept of union is essential to understanding American politics from before the American Revolution until well after Reconstruction. Moreover, Kersh's careful history shows that the concept of union illuminates a surprisingly large number of issues in American political development.

In order to trace the history of the idea of union in America, Kersh employs various methods. His innovative starting point is a quantitative coding of a large sample of American newspapers between 1750 and 1900, supplemented with diaries, congressional debates, and presidential speeches. The book is not organized around this quantitative data, but the database helps to provide a graphic overview of the usage of the term and a helpful starting point for the analysis. The bulk of the book shifts expertly from broad-ranging discussions of this primary data, to close textual analyses of such leading figures in American political thought as Daniel Webster, Frederick Douglass, and Abraham Lincoln, to lessons drawn from a vast secondary literature.

The first chapter examines the "rhetorical genesis of American political union" from the mid-eighteenth century through the revolution. The colonial usage of union largely revolved around religious invocations of the concept in support of bonds of fellowship that were to extend across geographical and even denominational boundaries. This religious usage helped popularize the term, while linking up with more explicitly political developments. The revolutionary period displayed an interesting transformation of the term, as calls for an independent intercolonial union displaced British suggestions of an Anglo-American union on the Scottish model.

Chapter two examines the theory of "political union" in the early republic, from the founding through the 1820s. This chapter introduces the notion of union as compromise, occupying a "vital balancing role" between nationalists and localists and related divisions over a host of particular political matters. The chapter includes an extended examination of James Madison's theory of political union, which emphasized the value of union, "a restrained sense of mutual affection" among the citizenry, and a well-designed representative government (p. 80).

Chapter three takes up the problem of national unity during the period from the 1820s through the 1850s. This period includes a "democratization of national union" with greater emphasis on popular sentiment, mass interests, and ethnocultural appeals; Kersh discusses the expanding activity of the national government during this period and gives extended consideration to such important figures as Daniel Webster, Ralph Waldo Emerson, and John C. Calhoun.

Chapter four traces the rise of "moral unionism" in the North during the Civil War. The chapter consists largely of a close analysis of the unionist ideals of Douglass and Lincoln and tracks the effort to provide a positive moral mission and reformist impulse for the wartime goals of preserving national integrity.

Chapter five examines the complicated rhetoric of union during the Reconstruction period. In contrast to accounts of the period that emphasize the rise of a new nationalism or strong central state, Kersh finds the reemergence of "old-style unionist arguments" that prioritized "renewed national unity" (p. 200). The South quickly recovered the rhetoric of a particularly sentimental unionism, and this discourse of reconciliation proved more resonant in the postbellum North than the continued Radical Republican call for perfecting the union.

Chapter six considers the decline of union as an important political concept in the last decades of the nineteenth century. Kersh argues that African Americans were the first to abandon the concept as they saw white usage implicitly or explicitly exclude them from the idealized union. In the 1880s, the term increasingly was used to refer to labor unions, even as mass immigration and economic dislocations subverted the ideal of a shared political union. In the 1890s, "America" replaced "union" as the preferred locution, as "Americanization" became a concern at home and the projection of national power became a goal abroad.

Kersh provides a fascinating and useful political and intellectual history of the concept of union in the United States, and his account will be of great interest to students of American political thought and nineteenth-century American politics.

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FRANCIS G. HUTCHINS. *Tribes and the American Constitution*. Brookline, Mass.: Amarta. 2000. pp. 273. \$27.50.

This book by Francis G. Hutchins takes on the most important question in American Indian history, politics, and law: what is the relationship between the Indian tribes and the American nation? It is a masterful history, raising important questions and engaging in a highly nuanced study of the original texts that define the place of the Indian tribes within the American state. The thesis, stated in the first sentence, is that "modern federal law and current historical schol-

arship reflect serious misunderstanding of the relationship between tribes and the Constitution," casting the book as revisionist history that challenges existing paradigms—and the existing law and history of this relationship. The paradigm is best known in Indian history as Chief Justice John Marshall's oxymoronic "domestic dependent nations" language from *Cherokee Nation v. Georgia*, an 1831 case that now underlies most federal Indian law.

The *Cherokee Nation* case does not stand alone but is at the center of Marshall's Indian jurisprudence. This jurisprudence has generated extensive—and inconclusive—historical and legal analysis. It is not clear, for example, exactly how Marshall derived his analysis, but it is perhaps best attributed to his role as the "great federalist" who simply saw the necessity of taking all jurisdiction over Indian tribes from the states and giving it to the federal government in order to allow the national government to control the development of the vast public lands lying west of Ohio. Indeed, this political analysis was present at the time of *Worcester v. Georgia*, the next year, when the states-rights President Andrew Jackson uttered his famous "John Marshall has made his decision, now let him enforce it" edict. Ironically, while the Cherokees won the case, they lost their lands, and many lost their lives as they were removed to the Indian Territory a few years later. But Marshall's legal analysis remained as the source of federal Indian law, an analysis that relegated Indian tribes to an inferior, protected status as "children," not in the literal sense but as dependants, "wards" of the United States. This language took on a life of its own in the subsequent 170 years of American history.

This is the paradigm that Hutchins challenges. He is not the first to do so: most critics of American Indian law begin by challenging Marshall's analysis. But Hutchins presents the clearest, most carefully thought out, and best organized criticism of the place of the Indian tribes in American legal history. His central thesis is that there was always a different understanding, of two different peoples developing *together* as one nation, under a common sky, and a common flag—brothers and sisters rather than fathers and children. This model has also been described as the "two row wampum"—two rows of beads, running side by side, the length of a wampum belt and signifying the binding of two distinct peoples—by Robert Williams in *The American Indian in Western Legal Thought: The Discourse of Conquest* (1990), an analysis that Hutchins endorses.

Hutchins, however, puts much more emphasis on in the founding years of that relationship in the American republic. This is well-trodden historical territory and very difficult to analyze because so many different kinds of relationships between Indians and whites occurred in eighteenth-century North America. The role of Indians in the Revolutionary War is an example of the problem. The argument here is that the idea that Indians supported the British because of their paternalistic colonial policies is only partially true. Other Indian tribes had longstanding relationships with the

American colonists, lived with them, and entered the war on their side, intending to secure for themselves a place as Indian nations in an independent United States. In the course of the war, they fought side by side with European Americans, under General Washington who saw his Indian allies as "brothers" and attempted to incorporate that understanding into law and policy.

This difficulty continues through the early political history of the United States. The complex negotiations over, first, the Articles of Confederation and then the Constitution reflected a wide range of views on the place of the Indian tribes in America. Hutchins proceeds through this material with considerable imagination and creativity, providing a first-rate account of the place of the Indian tribes in the Constitution. The two places the Indian tribes appear, in the "Indians not taxed" clause delineating who is counted for purposes of representation in the House of Representatives and the "Indian commerce clause," are both contextualized within the political debate of the time and presented as inconclusive compromises. Doubtless that is what they were, but Hutchins makes a number of insightful arguments that attribute much more meaning to this politics.

In sum, this is a well-written and well-argued analysis, concluding with a challenge to Americans to redefine the place of Indian tribes in their own country, to move beyond the "domestic dependent nation" analysis (which is also effectively critiqued) and toward a relationship that is more equal and more just.

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MICHAEL J. GERHARDT. *The Federal Appointment Process: A Constitutional and Historical Analysis*. (Constitutional Conflicts.) Durham, N.C.: Duke University Press. 2000. Pp. ix, 400. \$37.95.

Michael J. Gerhardt, a respected scholar of presidential impeachment and constitutional theory, purports to offer a comprehensive examination of the origins, structure, and processes by which federal judges have reached the bench. His study also includes an analysis of the major players (presidents, senators, party officials, and the attorney general) in that process, along with suggestions for future reforms. Gerhardt gives particular attention to some of the schemes touted as ways of reducing the partisan and political influences on the federal judicial appointments process. These suggestions include proposals that judges no longer serve during good behavior (i.e. are not subject to term limits) and that the confirmation of Supreme Court justices be based on a supermajoritarian vote of the Senate. Wisely, Gerhardt labels both propositions "problematic" (p. 291).

Historians of the federal judiciary will find Gerhardt's work of interest because it emphasizes, especially in the early chapters, the concept of historical

institutionalism. Gerhardt argues that scholars of American law and politics have been far too willing to view the appointment process in personal rather than institutional terms. Thus, the story of that process has been reduced to how well either a particular president or senator infused his or her beliefs into a particular judicial appointment. Instead, Gerhardt insists, students of the process would be well advised to approach it from an institutional perspective. "Historical institutionalism," he writes, "suggests that every president faces a peculiar combination of external and internal challenges in the course of trying to put his individual stamp on the office of the presidency if not also on the Constitution and the nation's course in history" (p. 87). The measure of a president's success is in how well he managed those circumstances in meeting the administration's long and short-term objectives for the federal judiciary and the Constitution. The absence of conflict, Gerhardt concludes, is not the appropriate measure of institutional success. Andrew Jackson and Ronald Reagan, for example, endured recurring struggles with the Senate over their judicial nominees, but in so doing they defined "themselves and their opposition" and cleared the way for "significant aspects of their constitutional visions of lasting change" (p. 93).

Gerhardt astutely combines his excellent insights into the development of American constitutional law with a keen understanding of politics, especially twentieth-century politics. His book, therefore, has a singularly practical quality that complements its otherwise scholarly tone. Perhaps Gerhardt's most important finding is that informal arrangements have been and remain more important than formal, constitutional requirements. As a result, he concludes that the first step to wisdom about judicial appointments begins with an appreciation of the impact of senatorial courtesy, logrolling, individual holds, blue slips, the views of sitting judges and justices about potential appointees, and interest-group lobbying rather than with an invocation of the constitutional clauses covering the appointment process. Such a perspective is important, he concludes, because it holds the promise to bring clarity to what is usually treated as an otherwise mysterious process and legitimacy to the judges whose actions shape the Constitution.

While a valuable contribution to the literature on the federal judiciary, the book falls far short of being the comprehensive analysis of the appointment process that the publisher touts. Perhaps its most glaring weakness is in the sections dealing with the early history of the process. Gerhardt draws on stale, older studies and ignores some modern monographic scholarship, such as my own *The Politics of Justice: Lower Federal Judicial Selection and the Second Party System, 1829-1861* (1979), and a host of historically based political science and law review articles. These studies have limned the broad outlines of Gerhardt's thesis and rest on a foundation of original research far beyond the author's. Indeed, Gerhardt cites no primary sources, such as the abundant records relating to

federal judicial appointments in the National Archives and manuscript collections of many of the major actors. He also misses many of the sophisticated analyses of the backgrounds of the judges and justices whose trajectory to the federal bench he proposes to explain. Even more disappointing is that he ignores in his copious citations any reference to works that have drawn on these sources and analyses. In short, although the book advances provocative ideas about the federal judicial appointments process, readers schooled in the existing literature on the nineteenth-century political and social history of the federal judiciary will find Gerhardt's treatment suspect in its conclusions because it is often casual in taking account of the scholarship of others.

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TIMOTHY JAMES LOCKLEY. *Lines in the Sand: Race and Class in Lowcountry Georgia, 1750-1860*. Athens: University of Georgia Press. 2001. Pp. xviii, 280. \$45.00.

In this work on lowcountry Georgia, Timothy James Lockley assesses "the relationship between African Americans and those white people who did not own slaves" (p. xvi) by examining social contacts, economic networks, criminal encounters, and shared religious experiences. Rejecting the longstanding argument that racism and white solidarity formed an insuperable barrier that united whites, slaveholders and nonslaveholders, against African Americans, Lockley argues that lowcountry nonelite whites (nonslaveholders) interacted with African Americans in a variety of complex ways, at times to the dismay of the elite. Furthermore, the author states that the history of social contacts between lowcountry nonelite whites and African Americans contradicts the theories that race shaped whites' behavior toward African Americans and that poorer whites were the most race conscious of all.

Lockley devotes more time to economic interaction between lowcountry nonelite whites and African Americans than he does to the other topics. He discusses the growth of independent production by slaves in the lowcountry, marketing strategies slaves employed, and slaves' subsequent development of trading relationships with white shopkeepers (nonslaveholders) before shifting to trading on Sunday, the major issue dividing whites in Savannah. Agitation over this issue led to a split in ranks as the elite refused to change the work schedule of their slaves to accommodate opponents of trading on Sunday and shopkeepers (nonslaveholders) refused to stop purchasing from slaves on whom they depended for working capital. Shopkeepers, moreover, were willing to forego racial ties with the elite for economic gain, and the elite was powerless to prevent economic cooperation between nonslaveholders and slaves.

The remainder of the study deals with criminal encounters and religious experiences. In a chapter

entitled "Violence, Theft, and Plot," Lockley discusses the complicity of lowcountry nonelite whites in criminal activity and violent acts of African Americans, mainly slaves, and their aiding and abetting slave runaways, arguing that these actions demonstrated nonelite whites' rejection of elite whites' social ethics. In the final chapter of the work, "Praying Together," Lockley argues that although African Americans were permitted to worship with whites in biracial churches and to establish independent black churches, and although evangelical denominations blurred somewhat the social distinction between whites and African Americans, the churches' support of established hierarchies ultimately limited biracial identification.

Lockley frequently states that lowcountry nonelite whites did not distance themselves from African Americans; instead, they interacted with African Americans in a complex variety of relationships, sometimes as social equals. Social barriers existed, however, and the legal status of "white people" permitted lowcountry nonelite whites to ignore those barriers without posing a threat to the social order of the lowcountry. The barriers were thus "lines in the sand," "lines that were impermanent, movable, and vulnerable but still existed" (p.165). Moreover, antagonisms existed between whites and blacks.

The work is based upon extensive research in primary sources, and each chapter is well documented. In spite of these strengths, however, the book has glaring shortcomings. It is a bit repetitious and at times has little regard for chronology, as the discussion moves back and forth among the colonial, early national, and antebellum periods. The terms nonelite, nonslaveholder, and shopkeeper used to depict the heterogeneous group of whites that are the subjects of this study are just as inaccurate as the ones the author rejected: common white, poor white, plain folk, and yeoman. Furthermore, Lockley committed factual errors in his discussion of the black church in Savannah, stating that Henry Cunningham left First African Baptist Church and joined First Baptist Church (the white congregation) after he was passed over as pastor of Second African Baptist Church (1802), and that the Sunbury Association dissolved First African Baptist Church as a result of the William Campbell controversy (1832). Andrew Bryan, founding pastor of First African Baptist Church, dismissed Cunningham from First African Baptist Church in 1802 to effectuate his appointment as founding pastor of Second African Baptist, and First African Baptist Church has existed continuously since its founding on January 20, 1788. Finally, much, if not most, of the wealth of secondary sources in the bibliography is not in the notes.

The wealth of manuscript collections and the discussion of coastal Georgia counties south of Chatham County (Savannah), an area historians tend to overlook, are the study's primary strengths. The work, moreover, successfully challenges misconceptions pertaining to race and class in pre-Civil War lowcountry

Georgia, hence contributes to the corpus of knowledge on the American South.

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ROSS FRANK. *From Settler to Citizen: New Mexican Economic Development and the Creation of Vecino Society, 1750-1820*. Berkeley and Los Angeles: University of California Press. 2000. Pp. xxiv, 329. \$45.00.

Ross Frank's carefully researched and well-written book covers a crucial period in the history of Spanish New Mexico and makes a substantial and welcome contribution to the growing field of borderlands studies. More thoroughly than any previous scholar, he documents the economic growth that began in New Mexico about 1780 and continued until the beginning of Mexico's War of Independence in 1810. He attributes this dramatic transformation to a number of interrelated causes, chief among them the defeat of Comanche Chief Cuerno Verde and his followers in 1779 and the definitive Spanish-Comanche alliance formalized in 1786. Burgeoning trade with Chihuahua and Sonora, government policies that increased New Mexicans' access to credit and bills of exchange, the provision of goods to the Comanches and other pacified Native groups, and the demands of frontier defense posts all stimulated New Mexico's economy as never before.

Frank then traces the social and cultural shifts in Hispano villages and in Pueblo communities that resulted from these economic changes. The line between "Spanish" and "Pueblo" sharpened, as the two groups no longer relied so heavily on one another for mutual defense. Pueblo numbers stagnated while those of Hispanos expanded rapidly, and rates of intermarriage between the two groups declined. The relative symbiosis forged against a common enemy during the 1760s and 1770s gave way to intensified competition for resources, as Hispano *vecinos* founded new communities and steadily encroached on Indian lands. In sum, "social interaction between Pueblo Indians and vecinos became a casualty of the structural changes in the New Mexican economy" (p. 122).

Meanwhile, distinct artisan traditions emerged among Hispanos and Pueblos. *Vecinos* effectively cornered the textile market and developed new styles, such as the so-called "Rio Grande blanket," that became popular throughout northern New Spain. Often without formal ecclesiastical permission, *vecinos* also began building their own churches in the new communities they founded. Even though the Franciscans were turning away from their former drive to acculturate and defend the Pueblos and concentrating their energies instead on the *vecino* communities, there were never enough priests to go around, and popular religiosity flourished in groups such as the *penitentes*. *Vecino* carvers and carpenters manufactured *santos* and other forms of religious art as well as trunks, furniture, and other utilitarian items, adapting

traditional Mexican styles to locally available materials. For their part, Pueblo artisans specialized in pottery, producing great quantities for export through *vecino* middlemen, and Native communities developed distinctive designs that became familiar to art historians and collectors down to the present. In form and in decorative motifs, these new styles reflected the tastes of non-Indian consumers in distant markets, and the quality of workmanship declined as the volume of production increased. In Frank's analysis, material culture reflected the changing social and economic world of late colonial New Mexicans, Pueblo and Hispano alike.

The author's use of the word "citizen" as a synonym for "*vecino*" in his title might lead readers to expect an explicit discussion of New Mexicans' political loyalties during these years when national identities had begun to coalesce in the westward-moving United States and even to some degree in Mexico. If so, they may be disappointed, for Frank concentrates the transformation of *vecinos* "from settlers laboring for survival on a difficult frontier to citizens living in a social and cultural context they had worked to create" (p. 176). The *vecinos* we meet in these pages clearly looked more to communities seen than to those imagined, and their worldview expressed itself most explicitly in the blankets, *santos*, and furniture they crafted.

Frank's work has profound implications for anyone interested in the political changes that came to New Mexico after 1820, however. Although *vecino* society incorporated detribalized, hispanicized Indians (*genízaros*) and people of mixed ancestry (*castas*), their emerging self-identity increasingly differentiated them from their Pueblo neighbors. They had begun to privilege the Spanish side of their inheritance even before the blatant racism of the North American invaders gave them added reason to stress their European ancestry. At the same time, the Pueblos also devised new forms of cultural resistance. Future scholars working on questions of local, ethnic, cultural, and national identities in the borderlands will do well, therefore, to consider Frank's findings.

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PATRICIA CLEARY. *Elizabeth Murray: A Woman's Pursuit of Independence in Eighteenth-Century America*. Amherst: University of Massachusetts Press. 2000. Pp. xii, 279. \$29.95.

The subject of this biography, Elizabeth Murray, was born in Scotland in 1726, migrated to live with her older brother in North Carolina in 1739, and settled in Boston as a single woman in 1749. In Massachusetts, she was a shopkeeper for a decade, married three times, experienced the American Revolution firsthand, and died in 1785. Patricia Cleary has used Murray's own letters, as well as those written by family and

friends, as the foundation for a description of the life of this "extraordinary ordinary woman."

Three major themes surface repeatedly in Cleary's narration of Murray's story. First, geographic migration played an essential role in allowing Murray and her relatives to maximize their opportunities and move up the socioeconomic ladder. Her oldest brother, James, led the way by moving to the American colonies in 1735 to seek wealth from land and trade, and over the years many other family members made transatlantic journeys in pursuit of various social and economic opportunities. Elizabeth Murray's own choice to live in Boston allowed her to achieve a level of success and economic security that she probably would not have enjoyed in Scotland, England, or North Carolina. Second, Cleary shows how family connections, as well as a sense of shared responsibility for younger relatives, played an important role for the Murrays. Elizabeth herself took in several of her nieces in order to provide them with mentorship, parental guidance, and a useful education, and she supplied other family members with money, homes, contacts, and training.

The third theme, Elizabeth Murray's quest for independence, is the most important, as suggested by the subtitle of the book. Throughout her life, Murray sought ways of achieving autonomy in a society that severely limited women's rights. She asserted a degree of political independence in the way she dealt with the revolutionary turmoil of the 1760s and 1770s, but, even more notably, she managed to attain a fair amount of economic independence, in large part through her own financial initiatives. Her brother James's commercial connections and money helped her establish her shop in 1749, and then her single status allowed her the freedom to make her own business decisions and contractual arrangements. Within a few years of its founding, her retail enterprise was well established; in 1753 her shop contained imported merchandise worth seven hundred pounds sterling. When Murray married at the age of twenty-nine in 1755, her business became a joint enterprise with her husband Thomas, although the rules of coverture meant that her property now belonged to Thomas and written commercial arrangements were undertaken in his name rather than hers. After Thomas's death in 1759, Elizabeth resumed full control of the establishment and continued to operate it until her second marriage, to a very wealthy older man, in 1760. Even then, a prenuptial contract gave her continuing control of her own property. Cleary's description of Murray's business, which is enhanced by a broader discussion of the experiences of female shopkeepers in Boston at this time, makes a particularly important contribution to our understanding of the economic and social history of colonial America. Notably, she manages to describe the lives of a number of unusually successful women without ignoring the constraints that limited opportunities and economic power for the vast majority of women in this period.

Scholars of early American history will find much of interest in this rare book-length portrait of an eigh-

teenth-century woman. Cleary tells an engaging story. The quotations from eighteenth-century letters keep us as close as possible to the perspective that Elizabeth Murray had at that time and help us to avoid superimposing a present-day view of the world onto her and her contemporaries. There is value in understanding their world as they understood it themselves. Cleary provides a broader context by bringing in other women's and men's stories where relevant, so we end up with more than one woman's story. Without a heavy theoretical or historiographical overlay, the stories illustrate many of the key issues and experiences of the time, such as immigration, trade and consumption, family and community, and the American Revolution, and thus make a useful contribution to scholarship on early American history.

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SUSAN BRANSON. *These Fiery Frenchified Dames: Women and Political Culture in Early National Philadelphia*. (Early American Studies.) Philadelphia: University of Pennsylvania Press. 2001. Pp. 218, Cloth \$47.50, paper \$17.50.

This book reflects the new historiographical interest in the political activities of American women before they got the vote. Even though the republic, in theory, excluded women from political life far more strictly than monarchical regimes, we now know that women did not have to wait for the Nineteenth Amendment to take a role in politics. Historians have been carving out an earlier political role for women, partly through redefining politics and partly by ferreting out the part played by women in quite traditional forms of politics outside of voting. Susan Branson's book is an interesting addition to our understanding both of the early republic and of the connection between women and politics. It should be read in conjunction with the richer *Parlor Politics* by Catherine Allgor (2000), which begins where Branson leaves off, with the move of the capital from Philadelphia to Washington, D.C.

Like other historians who have dealt with women in public, Branson makes much of the ceremonial role assigned to women and girls in the public processions that were such an important part of the visual rhetoric of the early republic. But young women dressed as "liberty" or presenting laural wreaths to George Washington are surely playing the same kind of public role as cheerleaders at a football game: they titillate the public, but no one is under any illusion as to what the main event is. However, the fact that Philadelphia, the nation's capital from 1791 to 1800, and then Washington, D.C., were political cities where women could be much in evidence at theaters, social gatherings, and Congressional debates in public galleries, made it more difficult for men in politics not to "remember the ladies." In such close contact with affairs of state, moreover, only the most determinedly domestic women could avoid acquiring some political opinions.

Branson concludes with the emergence of the "American political salon" in the 1790s, presided over by the wives of the social and political elite. Here women could join in discussing political issues and help to create the necessary glue of networking and alliances. They allowed women, insists Branson, "to achieve a foothold in the world of politics."

More than the American Revolution, it seems to have been the example of the French Revolution, and the intense political partisanship it engendered in the United States, which stirred some American women to rethink their own roles and possibilities. Both the expanding world of magazines and the revival of a flourishing theater scene in Philadelphia in the 1790s offered a forum where questions about women's nature and roles could be aired. Mary Wollstonecraft's *A Vindication of the Rights of Woman* (1792) was immediately reprinted in the United States and widely read and commented on. Journalist William Cobbett, who was rabidly anti-French, feared that American women might be catching the contagion. Those "fiery frenchified dames," he complained, were beginning "to talk about liberty and equality in good masculine style" (p. 72).

I am not convinced by Branson's claim that the developments of the 1790s, which she describes so engagingly, constituted "a watershed in the historical evolution of women's roles," or that they "laid the foundation for even greater female participation in the century ahead" (p. 149). As she admits only two pages earlier, "It is difficult . . . to draw a direct line from the activities of women in the late eighteenth century to those involved in the political and social causes of the nineteenth century. Though precedents were set, this did not immediately produce any consistently accepted public political role for women" (p. 147).

Exactly. Women's history still seems, not a history of precedents progressively built upon, but one of disjunctions, with each generation starting afresh to lay claim to new roles. Even with this rich history of political participation, when, as early as 1807, the property-holding and voting women of New Jersey were disenfranchised, no one, including the women themselves, seems to have objected. Eighteenth-century republicans liked to make a distinction between the corruption of monarchical politics, which was full of underhand petticoat influence and manipulation, and the purity of republican politics, conducted only by men in the open light of day. But it seems that republican men were, in fact, willing to wink at, even encourage, a certain amount of female "influence" in politics, as long as women did not presume to step up to the ballot box in a forthright masculine fashion.

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GAYLE V. FISCHER. *Pantaloon and Power: Nineteenth-Century Dress Reform in the United States*. Kent, Ohio: Kent State University Press. 2001. Pp. x, 262. \$24.00.

In 1825, Robert Owen, the patriarchal founder of the New Harmony community, proposed a radical dress reform for the women in his "Community of Equality" in southwestern Indiana. His ideas, and the ideas of other British Owenite reformers who joined him on his venture in North America, were simple: America was a city on a hill where he would establish the first intentional community wherein all people would be equal. There he and other reformers (after reading Charles Fourier on dress reform) promoted a new simple pants-and-tunic costume for community women. Most of the women living in the community, though, hated it—"a feather bed tied in the middle"—and refused to wear it.

Gayle V. Fischer points out one of the central ironies inherent in nineteenth-century American dress reform: it was a male patriarch who first crafted what women's liberation should look like, and it was the women in the community who opposed such "reform." Such is the story of New Harmony: Owen (and other European reformers, along with a number of Philadelphia intelligentsia) created a blueprint for women's liberation that included removing them from economic dependence on their husbands/fathers, educating them, and offering them new clothing. Such is also the story of class privilege, of course: the upper-class (predominantly male) designers of utopia imagined a liberation far different from that of the working-class women whose husbands joined the community in order to partake of an eight-hour work day and free education for their children.

Fischer does scholars a service by focusing on the Owenites in an early chapter of her book on dress reform in nineteenth-century America. For far too long, the Owenites have been left out of American history. Yet for communitarian scholars, Fischer's book will prove frustrating for a number of reasons. First, she is not attuned to the nuances of the Owenite experience, attributing all reform to Robert Owen himself. Indeed, Owen had many ideas, but he was joined at the community by other educated reformers (including some women such as visitor Frances Wright and resident Marie Fretageot). So to say that "the issue of dress highlighted the significant differences between Owen's motivations and those of the women who had to embody his ideas" (p. 39) is to miss class nuances: more people than Owen himself proposed dress reform. An upper-class elite proposed it; it was working-class women and lower middle-class women who hated the costume and all that it came to represent: a particular kind of secular reform being "forced" on them by infidels with crazy notions of women's equality, when they had come to the community for an eight-hour working day.

Communitarian scholars will also be frustrated by Fischer's tendency to cite reputable scholars alongside writers of historical romance or scholars who wrote so long ago that they were not able to use the wealth of new materials recovered in the past two decades. She makes no differentiation as to which interpretations

are more reliable and based on accurate records. One quick example: Fischer refers readers who want to know more about Wright's involvement with communal societies to O. B. Emerson's 1947 essay rather than to the numerous, excellent scholarly monographs and biographies that have come out in the past twenty years.

Fischer's monograph does a good job of delineating the disparate people advocating dress reform throughout the nineteenth century in North America, beginning with Owen and including health care reformers, farmers, spiritualists, and other religious patriarchs, including John Humphrey Noyes and his experiment in communitarian living in the Oneida Community. Thus, Fischer gives us an excellent context for the "turkish trousers" or the Bloomer costume that some of the women in the woman's rights movement wore briefly in the early to mid-1850s, and she nicely juxtaposes the Oneida Community's use of the costume (as social control) with the women in the woman's rights movement, who adopted it for political (as well as practical) use.

Fischer also points out the problems the new dress created for the women in the movement and how, within a space of about five years, all the reformers gave up wearing the bloomers. She relies too heavily, though, on Elizabeth Cady Stanton and Susan B. Anthony's *History of Woman Suffrage* (1881–1922). Thus, for scholars of the woman's rights movement, Fischer's treatment will prove frustrating as she does not question Stanton and Anthony's bias or dwell on the fact that only a few of the woman's rights reformers ever wore the bloomer dress (none of the older reformers such as Lucretia Mott or Ernestine Rose ever adopted it). She also gets some of the details wrong (Antoinette Brown Blackwell was Lucy Stone's sister-in-law, not her sister, for example).

Fischer ends her book by pointing out that the typical woman who put on pantaloons in nineteenth-century America was not a feminist, a member of an intentional community, or a member of the National Dress Reform Association; she wore them for practical reasons (p. 134). Excellent photographs of a series of farm women in the Midwest and West help to make her point.

CAROL KOLMERTEN
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EZRA GREENSPAN. *George Palmer Putnam: Representative American Publisher*. (Penn State Series in the History of the Book.) University Park: Pennsylvania State University Press. 2000. Pp. xv, 510. \$45.00.

Print culture in the United States expanded dramatically in the nineteenth century, fueled by improvements in technology and transportation, high literacy rates, and broad public interest in news and literary culture. Cheap newspapers and both fiction and non-fiction books enjoyed great popularity, setting sales records and prompting observers such as Alexis de

Tocqueville to marvel that Americans were inveterate and insatiable readers.

This well-researched book by Ezra Greenspan focuses on one aspect of nineteenth-century literary culture, examining the life and career of book publisher George Palmer Putnam. Putnam's career spanned from the 1830s to the early 1870s, and he was involved in a wide array of book-related ventures. He helped to develop the transatlantic book trade, created several popular book series (including the Library of Choice Readings and the Library of American Books), fought for international copyright legislation, and published the work of some of the most popular and gifted U.S. writers of the nineteenth century. There is much to recommend in this book. It is extraordinarily well researched, drawing on a broad array of original correspondence and nineteenth-century publications. Greenspan has a good sense of the larger context of book publishing in that era, noting the general growth of print culture (as represented by the rise of popular newspapers) as well as the transportation networks (both in the United States and across the Atlantic) that facilitated the distribution of books and other print materials. The author also appears to have a good sense of his subject; although he obviously admires much of what Putnam did in his career, he retains a balanced view of his subject's strengths and weaknesses. For example, Greenspan notes Putnam's tremendous energy but also observes that he was "never a man whose thinking was overly troubled by complexities or ambiguities" (p. 50).

This book provides valuable insight into the world of publishing and authorship in the nineteenth century. Putnam's link to many of his authors was, first and foremost, a business one. In the 1840s, he approached Washington Irving with an ambitious proposal to publish Irving's out-of-print works, primarily because Putnam saw this venture as a way to become a leading U.S. literary publisher. The venture was beneficial to both, giving Irving greater prominence and both men a great deal of money. One of the greatest strengths of this book is its attention to the business of publishing; Greenspan extensively details editorial/business decisions, notes marketing strategies (including the publication of extensively illustrated "presentation" books for the Christmas season), and reports on sales and profits. The author has a good sense of the dynamics of the U.S. print market, noting that success "was predicated on a large, relatively inexpensive supply of print products addressed to a large, broad-based audience" (p. 244). Overall, his work is an excellent source on the economics of book publishing in the nineteenth century.

The author also does a good job of capturing Putnam's personal relationships with many of his authors and others in the book-publishing world. Putnam turned his home into something of an annex to his office, often bringing guests home for dinner. Some writers even stayed with the family; Susan Warner spent three weeks as a houseguest at the Putnam

family home while she read page proofs for the first volume of *The Wide, Wide World* (1851). She was invited to return to read other proofs but demurred, finding the hospitality overwhelming.

Overall, this book is a strong biography that gives the reader a good sense of Putnam's own career. It is enriched by the author's sense of context and by an interesting writing style. It falls short, however, of its promise to present Putnam as a "representative American publisher." Demonstrating that he was truly "representative" would require a broader exploration of the book-publishing business than Greenspan details here. It is clear that Putnam was a prominent book publisher in his era, but there is really not a great deal of information about other publishers at that time to be found in this book; that broader picture is needed to make the case that Putnam was, indeed, representative. The author also seems to make some broader claims about the nature of literary culture in the nineteenth century, but these are developed only in bits and pieces as they pertain to Putnam's career or to the work of his authors. As such, the literary culture theme remains something of a sidelight. This is unfortunate, because it limits the significance of Greenspan's work. It remains an interesting biography of one publisher but falls short of providing a richer depiction of literary culture in the nineteenth century.

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IAN R. BARTKY. *Selling the True Time: Nineteenth-Century Timekeeping in America*. Stanford: Stanford University Press. 2000. Pp. xvi, 310. \$45.00.

This book is the story of an episode in American history that all U. S. historians know something about—the creation of standard time—but few will know the story as Ian R. Bartky tells it: a tale of nineteenth-century astronomers and their relationships with railroads, government, business, men, telegraph companies, clock makers, and even jewelers.

Bartky begins his book with a discussion of the railroad industry's timekeeping problem. In 1853, train wrecks caused by errors in timekeeping alarmed the country. At the time, a single track served railroad traffic in both directions. It was up to the conductors and the station manager to know the schedules for all trains using the same track and also to keep close watch on their own time. A Providence-bound train was running a few minutes late, but the conductor's watch told him he had five minutes to reach the double tracks into the station. He encouraged the engineer to speed up. The conductor of the out-bound train waited five extra minutes, but seeing no train approach, he gave the signal for the train to start. Less than a minute out of the station the trains collided. The in-bound conductor's watch had been wrong. At the ensuing inquest, the jury found the in-bound conductor guilty of manslaughter and the company guilty of negligence

for allowing the conductor to carry an inaccurate watch. Incidents like this one resulted in widespread public recognition of something railroad men already knew: the importance of accurate timekeeping. Railroad men went further: they wanted standard time.

The growth and importance of railroad travel created the practical need for accurate timekeeping, but railroad men were not the only ones who sought it. When the Erie Railroad hired an astronomer to keep "correct time from the heavens," it created an alliance between commercial and scientific timekeeping interests. Astronomers would later work with businessmen and compete with jewelers and telegraph companies. It is the multiple interests in accurate time and the competitions that emerged from them that Bartky explores in his carefully researched book.

Bartky has added depth to a seemingly familiar story. We knew that railroad men created standard time zones in 1883; what we did not know was how difficult that was to achieve. Bartky explains the problem of accurate and standardized timekeeping by describing early efforts to communicate time—from setting off explosives to dropping balls from tall buildings. The first effort to synchronize time in the United States was in 1844, when the Baltimore and Ohio Railroad's telegrapher in Baltimore sent the time to its station in Washington, D.C. making it clear that a "precise time signal could be sent great distances, but few saw much value in doing so" (p. 47). Even when the reason became obvious, the way was not so easy.

If time were to be synchronized, whose time was the correct one? This was a question for astronomers who had been trying to sell the idea of "true time" for much of the century. In the nineteenth century, astronomers were often required to support their observatories through some commercial endeavor. Selling the time was one scheme. Before the widespread accuracy of watches and clocks, observatories were the best source of time, and every observatory wanted its time to be the one sold to the public. It is surprising that at mid-century, jewelers were astronomers' most vigorous competitors, for the clock-making expertise of local jewelers was more visible to the public than work in an observatory. A jewelry store's clock could more easily supply the local time to the average person. Later, the observatories competed with telegraph companies, especially Western Union. Western Union received the accurate time free of charge from a government-owned observatory and then sold it to customers. In the 1870s, Western Union sent a daily time signal from Washington, D.C., to Chicago for an annual fee of \$300, to Boston for \$150, and to Pittsburgh for \$75. Local observatories were charging two to three times as much. By 1890, Bartky tells us, "astronomers viewed Western Union as a predator" and a number of their observatories would have closed if they had not been adopted by the growing universities (p. 196).

The complexity that the author finds in this story is both the strength and the weakness of the book. In his

desire to explore the details of the science and technology of time keeping alongside local and national politics, the story and chronology sometimes lack clarity. In his attention to detail, he sometimes loses sight of the larger story. Bartky offers a different view of the history behind standard time that nicely complements Michael O'Malley's *Keeping Watch: A History of American Time* (1990).

LINDY BIGGS
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HOWARD BODENHORN. *A History of Banking in Antebellum America: Financial Markets and Economic Development in the Era of Nation-Building*. (Studies in Macroeconomic History.) New York: Cambridge University Press. 2000. Pp. xxi, 260. Cloth \$59.95, paper \$22.95.

When bankers and stockbrokers appear at all in most American history textbooks, they usually get a bad press. They play starring roles in "wildcat" banking on the frontier, in periodic financial "panics," and in stock market crashes. They charge usurious interest rates to hard-working farmers and small businessmen, and they form a monopolistic "money trust" in the late nineteenth century to control and concentrate the expanding corporate sector.

Even specialists in antebellum banking history have stressed mainly its monetary roles. There has been lengthy debate about Andrew Jackson's "bank war" with Nicholas Biddle and whether Biddle's Second Bank of the United States was a proto-central bank (a precursor to the Federal Reserve) or Jackson's "pet banks" were inflationary. What little attention has been given to the credit-providing function of the banks generally portrays them as conservative, short-term lenders to established merchants, reluctant to meet the credit needs of either agriculture or industry. The work of Lance Davis and others on the gradual integration of a national financial market in the late nineteenth century implies that antebellum financial markets were local or regional, unable to mobilize savings across broad regions or sectors.

Howard Bodenhorn challenges these images and conclusions by studying the role of antebellum banks and brokers as credit-giving financial intermediaries and makers of efficient capital markets. He argues that bankers played an active ("supply leading") rather than a passive ("demand following") role in economic growth. By the 1830s, state-level money/income and other financial intermediation ratios were comparable to those Raymond Goldsmith has reported for successfully developing economies in the twentieth century. Antebellum merchants do appear to have received the largest share of bank loans, but artisan manufacturers and professionals received shares similar to their percentage of businesses listed in city directories. Regional interest rates were roughly equal and fluctuated in tandem over time, except for isolated frontier areas (the Mississippi Valley before 1830 or California in the

1850s), demonstrating that a reasonably efficient national capital market existed from the early 1800s. The Civil War bankrupted southern banks and left the South disintegrated from national financial markets for decades afterward.

As Bodenhorn himself repeatedly acknowledges, the quantitative evidence for his conclusions is imperfect and fragmentary. He estimates market interest rates in different locales by calculating chartered banks' net returns on earning assets. When rates in the Midwest appear higher than those in the East or South, he attributes that difference to the "monopoly" rates charged by the State Bank of Indiana; yet he has elsewhere argued that state restrictions on bank charters were largely overcome by the entry of private unincorporated banks, exchange brokers, and other currency issuers or credit intermediaries. His micro-research on the sectoral allocation of bank credit rests on the close study of the loan records of just four banks: one each in Watertown, New York; Petersburg, Virginia; Memphis, Tennessee; and Charleston, South Carolina.

His new quantitative evidence and statistical correlations alone would not convince most readers of Bodenhorn's broad and provocative conclusions. But his case is made much more persuasive by his effective reliance on the prior research of other financial historians and especially by his close examination of non-quantitative institutional and legal evidence. He shows that private bankers and commission brokers had started a market for domestic exchange before Biddle expanded the flows and reduced costs through the Bank of the United States branches beginning in 1822. Federal court decisions in the 1830s and 1840s made bills of exchange and promissory notes safer for distant investors, so that after about 1845 interest rates between regions differed only by the modest cost of arbitrage.

Bodenhorn's analysis of a blend of diverse primary and secondary sources persuades me that bankers and brokers occasionally played a dynamic Schumpeterian "innovative" role in antebellum economic development, particularly where they were state promoted and subsidized in the South and Midwest. But even where they "only" met market demands for credit from other more dynamic growth sectors (manufacturing, transportation, communication) and provided a reasonably efficient and integrated market for mercantile credit and the interregional and international mobility of savings, they deserve more praise for their supportive contributions to economic development than they usually receive. Bodenhorn makes that case quite well.

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PHILIP J. SCHWARZ. *Migrants against Slavery: Virginians and the Nation*. (Carter G. Woodson Institute Series in Black Studies.) Charlottesville: University Press of Virginia. 2001. Pp. xii, 250. \$38.50.

Historians' sensitivity to class, gender, race, and culture has over the past generation produced increasingly sophisticated portraits of the past. This is the case in regard to evaluations of the struggle over slavery that preceded the American Civil War. When Dwight L. Dumond published a history of the antislavery movement in 1961, he included a chapter on southern "migrants to the free states." In that chapter he focused on "cultivated men and women," all of whom were white and only a couple of whom were women. Now, in a book whose title is superficially similar to that of Dumond's chapter, Philip J. Schwarz focuses on ordinary men and women, black, white, and of mixed race, who left antebellum Virginia for the free states.

Schwarz has previously written on legal aspects of slavery in Virginia. Here he draws on an array of court records, government documents, census returns, genealogical materials, and secondary sources to seek meaning in the experiences of those "who consciously migrated against slavery" (p. 3). By leaving Virginia for the North, he asserts, these migrants "helped shape American character" (p. 4), weakened slavery in Virginia, strengthened antislavery forces, and found varying degrees of freedom for themselves. Throughout the book, Schwarz emphasizes interracial contacts among slaves and masters, among slavery's opponents, and particularly among family members. Other historians, such as Victoria E. Bynum, have used accounts of biracial families to explore social constructions of race. Schwarz is more concerned with the families' relationships to place and their genealogy.

One strength of Dumond's simpler approach to antislavery migrants was its clarity. A few elite white southerners, he maintained, relocated to the North because of their stated commitment to abolitionist principles and contributed substantially to the antislavery movement. There is no such clarity in Schwarz's more nuanced study. The heart of his book consists of four charmingly written chapters devoted to obscure individuals and families that left Virginia for the Northeast and Old Northwest. Their motives range from indecipherable to personal and their contributions to the antislavery movement are open to interpretation.

There is George Boxley, a white slaveholder who in 1816 was arrested for attempting to organize a slave revolt and who fled to Ohio and then to Indiana. Later Boxley aided fugitive slaves and passed his opposition to slavery on to his sons as well as to children he taught in local schools. Schwarz doubts that the motives for Boxley's antislavery activities will ever be known. There are George T. Gilliam and his wife Eliza Watkins Gilliam, light-complexioned free black slaveholders who in 1831 migrated from Virginia to Pennsylvania—leaving their slaves in bondage—and thereafter passed as white. Whether they cared about the rights of anyone besides themselves and their children is unknown.

A prominent theme in Schwarz's book is that African Americans who left Virginia could never com-

pletely escape the constraints slavery imposed on them. Schwarz most thoroughly develops this theme in his discussion of the Gist Settlements in Ohio. Wealthy Anglo-Virginian slaveholder Samuel Gist, who died in 1815, provided by will that his 350 slaves be manumitted and settled beyond Virginia's borders. Relocated to Ohio, expected to farm unsuitable land, mistrusted by stingy trustees, and resented by local whites, few of the former Gist slaves succeeded. Not truly free, they were migrants against slavery in a very limited sense.

Finally Schwarz devotes a chapter to Dangerfield Newby, a mulatto whose white father brought him, his mother, and siblings to Ohio in 1858 to free them. Shortly thereafter Newby, hoping to free his own wife and children, joined John Brown's band and died at Harpers Ferry. Even in so forthright an antislavery endeavor as Brown's raid, Schwarz suggests, family relationships, rather than abstract principle, motivated at least one of the participants.

The first three chapters of this book amount to an extended introduction to the four case studies. In these early chapters, Schwarz discusses many others who escaped slavery by leaving Virginia. He covers fugitive slaves, migrating free blacks and whites, the slaveholders' reaction, and the impact of fugitive slaves on the sectional struggle. These chapters are not as clearly focused as those on Boxley, the Gilliams, the Gist slaves, and Newby. They are, nevertheless, full of insights into what Schwarz shows was a complex and important phenomenon.

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JAMES SIMEONE. *Democracy and Slavery in Frontier Illinois: The Bottomland Republic*. DeKalb: Northern Illinois University Press. 2000. Pp. x, 289. \$38.00.

James Simeone provides bracing and sophisticated understandings of complex, unstably dynamic, and often paradoxical politics and politicized issues in early Illinois. He employs such established sources as Clarence Walworth Alvord, Arthur Clinton Bogges, and Theodore Calvin Pease and some newer standard sources, rich travel literature, autobiographies, newspapers, early histories, and published documents. Laudably, Simeone includes very recent works, and he deftly links findings to pertinent current political and social theories.

Simeone's clarion thesis is that, buffeted and befuddled by sweeping changes, poor egalitarian white males (mostly upland southerners, whom Simeone dubs "white folks") in Illinois sought to create a "bottomland republic" grounded on implicit white supremacy and explicit slavery. The "big folks," including abolitionists and northerners, opposed them. Battles between the moderate ("milk-and-cider") faction and the absolutist ("whole-hog") faction sundered the white folks. Generational tensions also flared. These fault lines and others and the thesis are embodied in Simeone's analysis of the fight in 1823–1824 over the

referendum to determine whether or not to call a constitutional convention to amend the state constitution to permit outright slavery (thereby overriding the intent of the Ordinance of 1787 and the Illinois State Constitution of 1818). Moderates among the white folks were decisive in defeating proconventionist forces.

Simeone bolsters his thesis with forceful and plausible, albeit not always convincing, arguments. His writing is generally lucid, his organization usually clear. He ably stresses some basic facts: fertile land lured most of Illinois's early settlers; fractious individuals, groups, cabals, philosophies, and assumptions in early Illinois heralded salient state and national change; cultural clashes racked early Illinois (even if Simeone scants Protestant-Catholic clashes); life, including political life, was tenuous and dicey, and alliances were transient. Extensive and valuable endnotes grace the work, as does a serviceable index. Fine illustrative materials abound, although the reader must surmise the date (post-1823 but evidently pre-1825) of one unusual map (p. 45), which suggests that by 1824 the northern half of Illinois experienced no frontier activities.

Some glitches and awkward writing mar the work. For example, John McLean was Illinois's first congressman, not Benjamin Stephenson (p. 92). Unlike laborers in the salines and other workers, "Dutch girls," "plowboys," and similar employees were usually hired hands (or "helps"), not indentured servants (p. 48). Christiana Holmes Tillson arrived in Illinois from Massachusetts, not Connecticut (p. 6). Buffeted by seasonal demands and sicknesses, many farmers displayed "uneven work rhythms" and apparent sloth, not just bottomland farmers (p. 53). Moreover, Simeone misleads by claiming the British "took over" the French country in Illinois in 1763 and "separated the colony from the rest of New France" (p. 102). Nebulous, shifting time frames and a population figure of 55,000 blemish one lengthy paragraph (pp. 137–138).

Gaps in sources further weaken the work. For example, Simeone often attributes thoughts and feelings to white folks. He cites memoirs, contemporary newspapers, and other sources, but he rarely cites white folks' manuscripts; most manuscript sources cited were generated by big folks, all males at that. Clearly, veins of white folks' manuscripts at the Illinois State Historical Library and the Illinois Historical Survey went unmined. Furthermore, valuable insights of Roger D. Bridges, Janet Duitsman Cornelius, Rodney O. Davis, Robert Howard, and Robert M. Sutton are largely and surprisingly absent.

Such flaws imperil the veracity of some conjectural assertions and arguments. For example, Simeone overstates new settlers' desires for equality; most almost certainly wanted equity, an opportunity for equality, equal opportunity, or perhaps a reasonably equal opportunity to become unequal. Few, however, were veritable egalitarians. Also, Simeone's views of republicanism probably insufficiently stress concepts of self-

restraint and the need to govern oneself and curb one's lusts. He overestimates popular dislike of the Ordinance of 1787 and the State Constitution of 1818, perhaps the era's most progressive state constitution. Languid generalities, sometimes repeated, and flashes of hyperbole further sap veracity. For example, by late 1823 were "mobs and fire in the night" truly "becoming routine" in Vandalia, the state capital (p. 3)? When over fifty-seven percent of the voters in the 1824 referendum opposed calling the constitutional convention, was their victory margin "slim" (p. 5)? Was "excess vegetation" so formidable that it strongly encouraged slavery? Finally, Simeone's apparently deterministic inclination and concomitant use of "determine" erode credibility (e. g. pp. 37, 41), and his overuse of the article "the" implies dubious inclusiveness or unanimity, as in "the people."

Nevertheless, skepticism concerning Simeone's thesis and the quality of the arguments and sources that support it does not gut the work. The book raises classic questions about legitimacy, power, elites, challenges to power, ends and means, social and political dynamics, and related significant matters. In addressing these questions, Simeone posits new ideas and provides grist for fresh debates about an ebullient and formative era, and he does so within national and international contexts. This work is valuable.

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CHRISTOPHER J. OLSEN. *Political Culture and Secession in Mississippi: Masculinity, Honor, and the Antiparty Tradition, 1830–1860*. New York: Oxford University Press. 2000. Pp. viii, 266. \$45.00.

Christopher J. Olsen's book on the political culture of antebellum Mississippi presents us with a compelling social history of politics. According to Olsen, the ideal of personal honor and violence among white men, born in frontier conditions, continued to hold sway in nineteenth-century Mississippi with the emergence of a slave society and a black majority. Fears of slave rebellions and the policing of slaves further inured white men to brutal violence. His description of violence-prone southern men is reminiscent of John Hope Franklin's classic *The Militant South, 1800–1861* (1956). Olsen argues that a "culture of masculinity" (p. 21) based on honor created a hierarchical society.

White Mississippians reconciled this hierarchical social ethic with democratic political forms at the level of local politics. Mississippi, which had frequent local elections throughout the antebellum period unlike older southern states such as Virginia and South Carolina, is a good test case for Olsen's argument. In the most well-researched and analyzed part of the book, he illustrates the deferential and ritualistic nature of local elections and planter influence on a democratic political exercise. Men of property, slaveholders, and planters dominated certain offices such as the boards of police and the crucial position of election

inspectors. His study of local politics is an original contribution to southern historiography and offers a corrective to the view that despite the existence of slavery, the southwestern states possessed an unalloyed white democracy.

County politics, Olsen informs us, had little to do with national issues, parties, and sectional ideologies. It was based instead on kinship, community, and neighborhood that gave rise to personal, face-to-face, non-institutional politics. The state's antiparty tradition was most visible at the local level, where political parties failed to nominate candidates. His conclusions here rely mainly on quantitative data gleaned from local election returns, but at times they seem to contradict his arguments. Some precincts, he acknowledges, maintained partisan continuity in elections. And if issues like the reopening of the African Slave Trade and the annexation of Cuba did percolate down to the county level, then they belie his contention that only party elites had ideology or ideological concerns regarding slavery. All politics, of course, was not local, and a similar analysis of state and congressional elections might have illustrated the importance of parties and political issues in Mississippi.

Party lines in most of the Lower South states broke down during the first secession crisis over the Compromise of 1850, and, no doubt, the rise of sectionalism and the decline of parties propelled these states toward secession. Olsen's claim that antiparty bias in Mississippi predated the 1850s is questionable. He contends that the state had an antiparty tradition based on an old republican fear of factions and furthered by nonpartisan local politics. In fact, most of his examples showing the weaknesses of the party system are from the decade prior to the Civil War. Except for South Carolina, Lower South states including Mississippi were a part of the second party system before its collapse in the early 1850s, and it was the reason they rejected nullification and stayed loyal to the party of Andrew Jackson in 1832. The absence of a two-party system in the 1850s in the Lower South made these states more prone to secession than the Upper South. But it was the sectional controversy over slavery and not some primordial fear of parties that led to their decline in the years before the Civil War.

Olsen's discussion of political events such as the Kansas controversy, the debate over the reopening of the African slave trade, the rise of the Know Nothings, and the coming of secession is disappointing. For example, he fails to explain the brief success of the Know Nothings except as an aberration of the state's antiparty culture. His tendency to reduce sectional controversies and secession to matters of personal insult for individual southerners is most troubling. Southerners, especially in states where slavery was widespread, were not merely insulted by abolitionists and free soilers but feared for the very existence of slavery. And if secession was a popular movement in Mississippi, why was voter turnout for the election to the secession convention only fifty percent compared

to antebellum voter turnouts that exceeded seventy and sometimes eighty percent? Finally, Olsen's attempt to ground secession in a political culture of honor rather than in differing sectional ideologies of slavery is unconvincing.

This book succeeds in its adept description of local politics but fails significantly to delineate the roots of secession.

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WALLACE HETTLER. *The Peculiar Democracy: Southern Democrats in Peace and Civil War*. Athens: University of Georgia Press. 2001. Pp. xi, 240. \$50.00.

"How democratic was the Democratic Party?" (p. 2) is the question that frames Wallace Hettler's engaging discussion of southern politics before and during the Civil War. His answer, essentially, is that it was not, because southerners could not sustain even limited democracy in a culture of inequality based on slavery and honor. Although southern Democrats promoted, and many truly believed in, the party's egalitarian image and message, its leaders were slaveowners determined to protect the institution and conditioned to seek, even expect, mastery over other men. Politicians bound by the public demands of honor conflated "political supremacy and masculine authority" (p. 58). In the end, Hettler concludes, the Democratic Party served the interests of planters at the expense of yeomen farmers.

According to Hettler, southern Democrats embraced Thomas Jefferson's ideology, which historians often term agrarian republicanism: a community of independent farmers possessing exclusive political rights, but also duties, which bound them together as virtuous citizens. To this conception was added Andrew Jackson's aggressive masculine image that prized personal honor and the raw physical courage and loyalty of ordinary white men, exemplified by the citizens' militia. These democratic ideals, however, sank in the region's culture of inequality. Increasingly worried about white unity and pressured nationally by the growing free soil movement, Democrats endorsed repressive, conservative, "whiggish" policies in the 1850s. These included internal improvements that most up-country yeomen opposed as threatening to personal independence. Ultimately, most planter-politicians feared that the nonslaveowning majority was unreliable and therefore refused to submit disunion to popular referendum.

After outlining these main themes in the first two chapters, the author uses the rest of the book to follow them—particularly that of democracy undone—through biographical sketches of five politicians: Virginia's John C. Rutherford, Georgia's Joseph Brown, Francis Pickens of South Carolina, Alabama's Jeremiah Clemens, and Jefferson Davis. The discussions

of Brown and Clemens are the most engaging, although all five offer new insights. Hettler's interpretation of antebellum politics largely concurs with historians who underscore class divisions; secession was engineered by nervous planter-politicians who feared an internal revolt against slavery and elite rule. During the war, latent hostility surfaced as opposition to Davis's centralizing policies, although the author stops short of blaming class conflict for Confederate defeat.

Hettler implies, or assumes, that most nonslaveowners would have opposed the planters' proslavery political culture, and secession, if given the chance. But he sidesteps the more vexing questions of if, or how, the voters perceived slavery as more than the institution itself—a set of issues and metaphors—or what influence yeomen had in the formal political process. He asserts that politics was "largely passive" for most men, "who are subject to the manipulation of elites" (pp. 4–5). But this relationship between elites and masses—vital to the arguments for undemocratic politics and elite manipulation—is examined only from the perspective of a few party leaders, and remains ambiguous. At times Hettler indicates, or suggests, that voters led politicians. Pickens "belatedly and opportunistically embraced immediate secession in November 1860," apparently to win favor with the voters (p. 113). Considering Unionist Democrats in 1860, the author casually remarks that "it is unclear whether they actually represented a popular majority" (p. 54). That seems like an important question. Finally, demonstrating that poorer whites were manipulated, subjugated, or intimidated entails other questions as well. Did voters actually believe slavery oppressed them (as Abraham Lincoln, and so many others, argued), or were the planter secessionists just paranoid? Political systems, of course, can seem (or be) undemocratic without poor people always being oppressed or dominated. Elite rule can be consensual. And what about all the Unionist planters in the Deep South who organized rallies and spoke against secession? In many places, the ultra-rich were diehard Unionists, they possessed money and economic power, and most of them had political experience. Voters had numerous chances to record a stand against disunion, most obviously in the presidential election of 1860, which Hettler does not discuss.

The book's greatest strength is its attention to change over time within the failed attempt to reconcile American egalitarianism with southern hierarchical values. Hettler confirms the pervasive influence of slavery and honor, which constituted a "culture of mastery and inequality" (p. 171) that shaped the region's politics. Although disputing several assumptions and particular conclusions, this reviewer finds the author's central argument compelling. Most of all, Hettler's fine work demonstrates the necessity of grounding any study of formal politics in a careful consideration of the society's most compelling ethics,

in this case the public imperatives of slavery, honor, and masculinity for southern white men.

CHRISTOPHER J. OLSEN
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LYDE CULLIN SIZER. *The Political Work of Northern Women Writers and the Civil War, 1850–1872*. Chapel Hill: University of North Carolina Press. 2000. Pp. 348. Cloth \$45.00, paper \$18.95.

Since Gerda Lerner and Aileen S. Kraditor published their pioneering works on the subject in the 1960s, many historians have studied the intersections between abolitionism and feminism. Their work sheds light on how the rhetoric of liberation raised abolitionist women's consciousness and poses important questions about the relations of power between white and black women involved in the movement. Despite white abolitionist women writers' racism, Jean Fagan Yellin argues, black and white women developed mutually rewarding collaborations and worked together "to establish an American sisterhood and to activate that sisterhood in the public arena" (see Yellin's introduction to Harriet A. Jacobs, *Incidents in the Life of a Slave Girl* [1987], p. xxxiii). Lyde Cullin Sizer contributes to our understanding of this sisterhood by examining the written works of a select group of the most popular and prolific nineteenth-century women writers. Whereas the subjects of this study rarely collaborated, Sizer argues that the great body of their collective work suggests that—like the Grimké, Sojourner Truth, and others who were more actively engaged in the work of liberation—they, too, shared a vision of an interracial, interclass sisterhood.

Sizer follows the careers of nine northern women writing during the Civil War era: Lydia Maria Child, Harriet Beecher Stowe, Fanny Fern, E.D.E.N. Southworth, Frances Ellen Watkins Harper, Rebecca Harding Davis, Louisa May Alcott, Gail Hamilton, and Elizabeth Stuart Phelps. The result is a collective literary biography, or "an intellectual portrait," as Sizer describes it, of two generations of women writers whose writing was shaped by the experience of war (p. 13). Whereas the older generation was dominated by the sentimental style and abolitionist sentiments of Stowe and Child, the political work of the younger generation ranged more broadly and was characterized by the more realistic styles of Harper, Davis, Phelps, and Alcott, who condemned racism, industrial capitalism, and women's disfranchisement, among many other things. Coming of age during the war, Harper, Davis, Alcott, Hamilton, and Phelps wrote with a new confidence, collectively creating what Sizer calls a "rhetoric of unity" that they hoped would bring women together across region, race, and class to shape the postwar nation (p. 12). If the influence of this literary sisterhood was limited and fleeting, Sizer suggests that it was the gravity of the issues they addressed that is to blame rather than northern women writers' limited ability "to see beyond their class or race" (p. 281).

The society in which Sizer's subjects wrote was deeply divided. When middle-class white women like Child (*An Appeal in Favor of That Class of Americans Called Africans* [1833]) and Stowe (*Uncle Tom's Cabin* [1851]) launched their antebellum careers writing in support of the abolition of slavery, they were acutely aware of the transgressive nature of women expressing their political opinions and often prefaced their political statements with apologies to their readers. As an African-American woman, Harper (*Iola Leroy* [1893]) understood even more profoundly the limits of her social power when she spoke and wrote in opposition to slavery and racism and encouraged African Americans to fight racial injustice. With less reticence than their predecessors, Fern (*Ruth Hall* [1854]) and Alcott (*Little Women* [1868] and *Work* [1873]) wrote in support of women workers, and Davis (*Life in the Iron Mills* [1861]) and Phelps (*Gates Ajar* [1868]) exposed the exploitations of industrial capitalism and condemned the economic suffering of women in the postwar era.

Inasmuch as nineteenth-century societal "fissures" inspired women authors to take up their pens and venture into the male terrain of politics, these divisions also circumscribed the vision and the influence of women's political writing (p. 281). Though the war vindicated their antebellum abolitionist writing, Child, Stowe, Alcott, and other white women writers, Sizer argues, prematurely abandoned the freedmen's struggle against racial injustice in the 1870s. Those who did not immediately abandon the plight of the freedmen made somewhat awkward attempts to envision a postwar society freed of racial prejudice. Both Child and Davis, for example, wrote postwar novels that embraced racial intermarriage, but in the end, Sizer laments, they fell victim to racial stereotyping and paternalism. Only Harper steered clear of stereotypes and consistently reminded her readers of their responsibility to fight racism. Yet class proved to be a limitation for Harper, who spoke more to the needs of an emergent black middle class with her assimilationist message than to the majority of freedmen. Alcott assumed that black emancipation was complete even before the end of political reconstruction, yet Alcott, Phelps, and Harper at times shared a vision of an interracial, cross-class, "loving league of sisters," as Alcott described it, that might have served as a useful model for postwar politicians charged with reconstructing the nation (p. 262). In the end, whereas each writer's individual myopia limited the transformative power of her rhetoric, collectively the political work of this group of women reminds us that an entire generation of Americans who came of age during the Civil War sought a greater meaning to that war than the sum of individual sacrifices. By daring to envision a postwar society undivided by race, class, or gender, Harper, Phelps, and Alcott helped their readers to see the constructive potential of their generation's most destructive war. And Sizer makes a convincing case for

why this body of wartime writing deserves a closer look.

With that said, however, there are two omissions in this book. At several points, Sizer compares the authors based on their readership: Southworth, Sizer tells us, enjoyed "a large working-class audience," and Alcott's was middle class (pp. 219, 252). It would have been useful to know more about how Sizer determined the composition of each woman's readership—other than by the language and the content of their published works—and to have a thorough comparison of the audiences of all the writers. Second, while Sizer begins with a chapter describing the domestic conditions in which women wrote, including Stowe's famous line describing the many domestic distractions kept at bay only by her "deadly determination" to write, Sizer does not describe the atmosphere in which women read (p. 20). We know that middle-class women talked and wrote letters to one another about what they read. Working-class readers were perhaps even more public about what they read, often sharing journals and reading out loud. With these writers commenting on the most important political questions of the day, a brief discussion of how their words were read, repeated, and translated into action would have been useful for understanding their cultural and political importance. These are minor criticisms, however, and they do not detract from the Sizer's overall success. Her book is a thorough and nuanced exploration of these authors' wartime writing. It will be of interest to historians and literary critics alike.

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SHAWN MICHELLE SMITH. *American Archives: Gender, Race, and Class in Visual Culture*. Princeton: Princeton University Press. 1999. Pp. xi, 299. Cloth \$55.00, paper \$19.95.

This book by Shawn Michelle Smith analyzes the multiplicity and interconnectedness of nineteenth-century representations of class, gender, and race. The book ambitiously tracks these themes across diverse media. As the author explains, she includes "popular, literary, artistic, and scientific discourses that called upon photographs to make claims upon identity" as well as "photographs themselves" (p. 7). To discuss such a variety of topics and media, the trade-off is the unity of the narrative.

Nathaniel Hawthorne's *House of Seven Gables* (1851) does double duty introducing Smith's argument. In the first chapter, she analyzes this "romance of middle-class ascendance" (p. 19) to illustrate that nineteenth-century female submissiveness and middle-class "interiority" replaced the Puritan ancestors' aristocratic "exteriority." The second chapter sees Hawthorne's critique of "blood purity" as parallel to the "conceits of blood purity, heredity, and inherited character [that] were rapidly becoming the categories of 'race' that most fascinated white middle-class Amer-

icans" (p. 28). Smith then discusses "biological racialists," who worked to define race as a fixed category used to justify discrimination and even slavery.

In chapter three, Smith turns to a variety of photographic practices, both European and American, to examine the production of visual representations of middle-class identity. The chapter is perhaps too broadly conceived, and middle-class imagery is defined largely in opposition to what it was not: the exteriority emphasized in overly literal "likenesses" associated with low art, mug shots of "the criminal body" from police archives supervised by Parisian police chief Alphonse Bertillon, blended images of the "criminal type" designed by the British founder of eugenics Francis Galton, and exteriority of a different type seen in the hundreds of self-directed portraits of the famed Second Empire beauty the Countess Castiglione. In each of these cases, Smith covers ground already well trod in photographic history, and she acknowledges the work of Allan Sekula, Alan Trachtenberg, Abigail Solomon-Godeau, and others. More unique are her analyses of little-known images of American models wearing photographs as studio advertisements and Alice Austen's masquerades, which Smith sees as challenges to normative gender representations. Smith presents all these images of "others" as evidence that the middle classes believed that photography both captured external facial types and mapped interior essences.

Yet Smith believes that facial maps were not the images that members of the middle classes sought for themselves. Though her argument is not fully developed with visual examples, Smith sees the middle classes drawn to more expressive portraiture, or works of "art" (pp. 54–57). To exemplify middle-class desire for interior expression she discusses *Camera Notes*, edited by Alfred Stieglitz (1897–1902), making a cultural trend out of a small, later artistic movement. Smith's emphasis on expression is consistent with her use of theories of middle class interiority. But the vast numbers of extant *cartes des visites* and other photographic portraits, as well as the research of photographic historians as varied as Richard Rudisill and Elizabeth Anne McCauley, suggest that the middle classes were concerned primarily with obtaining a "good likeness," through which physiognomy conveyed character.

In the fourth chapter, Smith returns to the question of what constitutes middle-class visual culture. She demonstrates that baby pictures and family albums, promoted by eugenicists such as Galton, reinforced middle-class identity as white. The next chapter examines the continuing significance of "bloodlines," when, in the face of explosive immigration, the Daughters of the Revolution promoted genealogies and North/South upper-class unity. Smith includes fascinating contemporary African-American critiques by Anna Julia Cooper, Frederick Douglass, and Ida B. Wells, who were concerned about the erasure of black and multiracial heritages.

Chapter six provides a lively discussion of how the "American Negro" photographs exhibited at the Paris Exposition of 1900 envisioned "a racially codified American identity." Looking at the intersection of race and nationality, Smith contrasts Francis Benjamin Johnston's Hampton Institute images, which "forwarded the American identity of Hampton students over their racial identities," with photographic albums selected by W. E. B. Du Bois that "suggested the African American could indeed be both an 'American' and a 'Negro'" (p. 158). Such representations challenged dominant culture; the Du Bois selections, she argues, "begin to enable white viewers . . . to see the cultural logics and privileged practices that reproduce racism" (p. 183). It is in analyzing how ideas of race are inscribed in cultural texts that this book is most successful.

Literature takes up the challenge to dominant culture in the last two chapters, which examine Pauline Hopkins's serialized novels for their resistance to dominant racial ideas and Theodore Dreiser's *Sister Carrie* for its challenge to traditional gender roles.

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RUSSELL F. WEIGLEY. *A Great Civil War: A Military and Political History, 1861–1865*. Bloomington: Indiana University Press. 2000. Pp. xxviii, 612. \$35.00.

Military historians, Allan Millett astutely observes, are often less military historians per se than historians of specific wars. That is to say, their research, published work, and presumably knowledge base tend to focus on a specific conflict. Russell F. Weigley is a formidable exception to this rule. In a scholarly career spanning over forty years, he has studied not only the length and breadth of American military history, particularly in his classic *The American Way of War: A History of United States Military Strategy and Policy* (1973), but also European warfare (e.g. *The Age of Battles: The Quest for Decisive Strategy from Breitenfell to Waterloo* [1991]). Central themes from these earlier works re-emerge here: first, the failure of Robert E. Lee's single-minded pursuit of a Napoleonic decisive victory; second, the rarity of such victories even in Napoleon's day; and, finally, the success of Ulysses S. Grant's "strategy of annihilation," by which he eviscerated the Confederacy through the exhaustion of its armies, the destruction of its war resources, and the demoralization of its white population through a series of destructive raids.

Does the book improve on James M. McPherson's *Battle Cry of Freedom: The Civil War Era* (1988)? In his bibliography, Weigley himself calls *Battle Cry of Freedom* "the best comprehensive one-volume history" (p. 567). How about Herman Hattaway and Archer Jones's *How the North Won: A Military History of the Civil War* (1983)? With equal generosity, Weigley writes that, despite some interpretative disagreements, "for comprehensive coverage grounded in a superb

knowledge of the history of warfare it is the best one-volume military history of the war" (p. 566).

Such verdicts would seem to leave little room for yet another single-volume history, particularly one confined to political and military events. And in some respects the book is a bit curious. It assumes too much background for the Civil War novice—the conflict, for instance, seems to emerge virtually without causation—while cognoscenti will find themselves flipping page after familiar page. Battle narratives in particular tend toward the pedestrian. The strength of the book lies partly in Weigley's ability to see the conflict within the framework of the larger history of warfare, and to draw conclusions that, while certainly subject to disagreement, are intelligent, incisive, and the product of a lifetime of study.

Further, Weigley goes well beyond *How the North Won* in his study of the vital political context. Hattaway and Jones mention the Emancipation Proclamation in passing on three separate pages. Weigley devotes a full chapter to emancipation, not to mention extensive sections on wartime Reconstruction, political dissent (North and South), and critical wartime elections and legislation. Thus we find not only concise assessments of the impact of the rifled musket—Weigley believes (wrongly I think) that it helped revolutionize Civil War battlefields—but more unexpectedly, his verdict on the role of the war in replacing "an unorganized nation by a highly organized society," as Allan M. Nevins once maintained (p. 308). Weigley disagrees, siding instead with Robert H. Wiebe's thesis that postwar America remained a "distended society" of island communities (p. 308).

Similarly, on the recently revitalized question of Confederate cohesion versus internal dissent, Weigley stands alongside the authors of *Why the South Lost the Civil War* (1986) and against Gary W. Gallagher in *The Confederate War* (1998). Gallagher thinks the Confederacy held up surprisingly well in the face of the northern onslaught. Weigley is, to say the least, sharply skeptical. However, he seems to agree with Gallagher about the importance of the war's eastern theater. Since the South could not prevail in a long struggle, its best chance of success lay in a series of major, demoralizing victories in the Richmond-Washington corridor as well as Maryland and Pennsylvania. Lee just about managed to achieve this.

As these examples suggest, Weigley often addresses the salient debates of the political-military contest directly in the text. The associated historians, their positions, and Weigley's own conclusions are quickly, deftly sketched. By the same token, almost everything in the book—names, ranks, casualty figures, and so on—has a refreshing precision about it, and the book is so well organized that one can locate any subject in a matter of seconds. In that sense, it makes a superb reference work as well as a useful interpretive synthesis.

The book has no illustrations, but that is of small consequence. More serious is the dearth of maps: just

eight for the entire book, two of which depict the battle of Gettysburg. This is a non-trivial omission, and it is unfortunate that Indiana University Press did not bestow upon a work of this stature the abundance of maps that good military history requires practically by definition. Even so, Weigley has performed a useful service. He has not supplanted McPherson's *Battle Cry of Freedom*, but he has contributed a readable synthesis that comfortably meets the goals it sets for itself, and which takes full advantage of the last decade of scholarship.

MARK GRIMSLEY
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PATRICIA C. CLICK. *Time Full of Trial: The Roanoke Island Freedmen's Colony, 1862-1867*. Chapel Hill: University of North Carolina Press. 2001. Pp. xxiii, 302. Cloth \$49.95, paper \$18.95.

Patricia C. Click has performed an outstanding service in rescuing the Roanoke Island freedmen's colony from historical oblivion. Like its better-known sixteenth-century predecessor, this "lost" colony left few actual footprints. Archaeological work has uncovered partial outlines of a military camp and shaken loose some artifacts, but little that occurred on the island during and immediately after the Civil War remains imprinted on public or private memory. With one important exception, Click's exhaustive research amply compensates for this loss, and her account of the colony's history will prove definitive. Equally important, her decision to shape a narrative accessible to specialists and general public alike is fully vindicated. A brief glance at the endnotes confirms that this well-written, informatively illustrated study entails no loss of research or analytical integrity. It is a valuable contribution to Civil War and emancipation scholarship; its simultaneous appearance in paperback should further guarantee that the story of the Roanoke freedmen's colony is restored to its rightful place in popular memory.

Click's introduction sketches the larger history of the contraband camps, facilities created to deal with the influx of runaway slaves whose impulse for freedom led them to the Federal lines. Camps developed in different ways, indicative of regional and local dynamics; uniting them was the freedmen's dependence upon government support and upon the efforts of northern missionary associations. The role of northern evangelicals in wartime and postwar relief work is well documented, but Click's sharply detailed investigation into the activities at Roanoke adds fresh perspective and evidence to this important topic. In the Reverend Horace James, who oversaw the establishment of the Roanoke colony as superintendent of freedmen in eastern North Carolina, Click has found an effective vehicle for investigating the complex, sometimes contradictory, beliefs and attitudes that permeated the North's relief efforts. Equally suitable is the history of James's cousin Elizabeth, who arrived

at Roanoke in October 1863. Employed as a teacher by the American Missionary Association, Elizabeth James remained on the island until February 1867, by which time its former white owners had reclaimed most of the colony's land. James wrote afterward that she had endured "a complication of experience upon [Roanoke's] evergreen shores" (p. 189). It is this "complication" that furnishes Click's history with its narrative texture.

Roanoke was unusual among contraband camps because from the outset it was envisaged as a permanent experiment in postemancipation community building, the only one in the state to be designated as a "colony." The founding belief of its architects was that the ex-slaves who came to Roanoke would be taught to support themselves economically, and to this end James and his coworkers plotted out a "New England-style village" on the north end of the island. Although he was convinced of the prime importance of land ownership, James's vision of free labor independence was not restricted to agriculture; he also envisaged the freedmen pursuing industrial arts. A steam sawmill, funded mostly from northern contributions, was the focal point of James's industrial project. Through the combination of free labor and technology, James hoped to validate his belief that the Roanoke experiment could become a starting place for a wider "regeneration" of southern society after slavery.

The need to earn one's keep was matched by the rage for education. Click notes that it was in the classroom that missionaries like Elizabeth James developed "their most intimate connections with the freedpeople" (p. 105). Tensions ensued as teachers sought to impose a New England school model, in which religious and secular learning were combined as one, on their newly liberated charges. One consequence was the appearance of an undetermined number of nonaffiliated schools, with instruction given by "colored teachers." What precisely prompted this development is unclear, as Click's mighty efforts regrettably have uncovered very few African-American sources from this time. On numerous occasions, as the historian herself laments, "the asymmetry of the nineteenth-century voices" (p. xvi) prevents a fuller understanding of black reactions to the efforts that were being made to turn the former slaves into productive, educated, and socially responsible Christian citizens.

The other vital strand to Click's history is the impact of the war itself on Roanoke's fortunes. Military movements in eastern North Carolina had a volatile effect, with the colony's numbers being regularly swollen by new influxes of refugees. Click also describes the ambivalence, and sometimes the downright disrespect, with which even well intentioned Federal military personnel dealt with the Roanoke inhabitants. In March 1865, the colony became the responsibility of the Freedmen's Bureau, increasing African-American dependency; and by the fall of that year, when James resigned, Roanoke's future as a community of independent black proprietors had already fallen victim to

President Andrew Johnson's restoration agenda. Although a small group of black families remained on the island, providing the core of a thriving twentieth-century community, the majority of land had reverted back to its original owners, and James's dream of a "happy commonwealth . . . of prosperous and virtuous people, of varying blood, but of one destiny" (p. 205) was, for the time being at least, lost.

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ROGER H. TULLER. *"Let No Guilty Man Escape": A Judicial Biography of "Hanging Judge" Isaac C. Parker.* (Legal History of North America, number 9.) Norman: University of Oklahoma Press. 2001. Pp. xii, 210. \$27.95.

Roger H. Teller's book is what he calls a "judicial biography" of Isaac Charles Parker, the chief judge of the United States District Court for the Western District of Arkansas from his presidential appointment in 1875 until his death in 1896. The court, sitting in Fort Smith, had jurisdiction over crimes committed by United States citizens in eighteen western counties of Arkansas and in unorganized Indian Territory in Oklahoma.

During Parker's tenure, his court docketed 12,031 criminal cases, resulting in 8,781 convictions. Roughly twenty-five percent of the cases involved serious crimes. Grand juries brought in 3,942 true bills for capital offenses. Following jury convictions, Parker sentenced 161 men to death, seventh-nine of whom hanged, as many as six at a time in highly publicized and well-attended public spectacles. With some reason, Parker gained a national media reputation as the "Hanging Judge." The only recourse to a Parker death sentence was executive clemency until 1889, when Congress gave the Supreme Court appeal responsibilities.

Parker, born in Ohio in 1835, read for the law and moved to the frontier community of St. Joseph, Missouri, gaining admission to the state bar in 1859. In the Civil War decade, he served in the Union forces, married, and held elected offices as city attorney of St. Joseph, county prosecutor of surrounding Buchanan County, and judge of a judicial district in northwest Missouri. In 1870, running as a Republican, he won the first of two terms in Congress, where he attracted attention as an advocate of the rights of the American Indians, proposing failed legislation to make them eligible for land under the Homestead Act. After losing a bid for reelection in 1874, he successfully lobbied the two Arkansas senators and President Ulysses S. Grant for a job as a federal judge.

Parker cleaned up public corruption in his district court and quickly stamped himself as a no-nonsense, pro-prosecution judge. He made arbitrary decisions, such as ruthlessly clearing crowded dockets. He worked closely with and influenced the conduct of the U.S. District Attorney and the U.S. Marshal. On the

bench, he browbeat lawyers, intimidated witnesses, and colored evidence in his jury instructions. He liked to hear himself talk and frequently gave over four hours of instructions. He had a very harsh judicial temperament and was prone to sudden bursts of ill temper. In his later years, he quarreled constantly with the Supreme Court and wrote acrimonious public letters of justification. He promoted western development, ruling against tribal rights in land cases.

The first of several books on Parker was *Hell on the Border* (1898), by C.W. Harmon, a Fort Smith hotel keeper and frequent jury member in Parker's court. A number of potboilers on Parker appeared in the 1950s: Homer Croy, *He Hanged Them High* (1952); Glenn Shirley, *Law West of Fort Smith* (1957), and J. Gladstone Emery, *Court of the Damned* (1959). All three works emphasized the criminal side of Parker and his court. Fred Harvey Harrington produced *Hanging Judge* (1951), the first academic assessment of Parker, which detailed his troubled relationship with the Supreme Court.

Tuller conducted extensive archival research. In Missouri, he used manuscript and state court records. At the National Archives-Washington, Archives II, and the National Archives-Southwest Region, he found valuable records pertaining to Parker's court, including official correspondence, case files, and record books. Tuller is the first scholar to make extensive use of this material. The correspondence is on fading tissue paper in letter-press books, and the case file documents are trifolios. The use of these fragile records lends authority to Tuller's interpretation of Parker's career.

On the basis of the new evidence, Tuller concludes that Parker, despite his outburst on the bench, was an effective and honest judge who followed the laws of the United States rather than the "code of the hills" in handing down sentences. Tuller states that "he often denied defendants fair trials and frequently ruled against the American Indians he claimed to support: he rebelled against correction by his superiors and humiliated himself with public displays of temper. But Parker established a lasting reputation for himself and for his court based on integrity and dedication to duty, and he fought lawlessness in his jurisdiction fiercely with all the resources available to him" (p. 162-63).

Tuller's admirably researched and clearly written book is a workman-like account that adds a new dimension to Parker's judicial career. It is an important contribution to the study of federal frontier justice.

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TED TUNNELL. *Edge of the Sword: The Ordeal of Carpetbagger Marshall H. Twitchell in the Civil War and Reconstruction.* Baton Rouge: Louisiana State University Press. 2001. Pp. xvi, 326. \$34.95.

Students of Reconstruction may have encountered Marshall H. Twitchell through his memoir, *Carpetbagger from Vermont* (1989), but Ted Tunnell's evocative biography succeeds in providing a more complete life of this under-appreciated figure. Twitchell's resumé reveals much about him. He volunteered for a Vermont regiment in the Union Army, serving as a soldier and a sergeant, passed the examination to be an officer in the U.S. Colored Troops and was wounded in action, took the opportunity to become a Freedmen's Bureau agent, and promoted public education in his adopted state of Louisiana, where he also held office as a Republican state senator. In Louisiana, Twitchell exemplified the northerner who invested in risky postwar land and agriculture ventures. Moreover, as a husband, father, and family patriarch he risked his relatives' lives and property by personifying Republican social, political, and economic changes brought on by Reconstruction. Tunnell does a remarkable job of describing and analyzing each aspect of Twitchell's life. He stresses the years 1865 to 1877 in Louisiana, emphasizing that the postwar violence visited upon Twitchell and his family stayed with him for the rest of his life, even after he gained an appointment as U.S. consul at Kingston, Canada, in 1877.

In writing this full biography, Tunnell encountered small gaps in Twitchell's life. The author fills such gaps well by taking a wider view of society, places, and developments. Describing Twitchell's Vermont regiment helps understand his early years in the Union Army. Persuasively establishing Twitchell's credentials as a Republican bent on social reform (certainly, former Confederates regarded him as a "radical"), Tunnell then presents a textured account of Louisiana's postwar economy, graphic images of the social pressures and racial tensions produced by emancipation, and evidence of the undeniable need for public schools across the state. Taken together, these bridges connecting Twitchell's salient activities give readers one of the best avenues into the swamps of Louisiana during Reconstruction.

Weaving his way through Twitchell's life, Tunnell addresses many issues. He skillfully dissects the hoary but persistent stereotypes of carpetbaggers, drawing on Twitchell's case to spell out the mixture of northerners' motives to settle in the South. Twitchell and his extended family created a community at Coushatta in a new county, Red River Parish. The intent to make a permanent home in Louisiana resulted in death for several friends and relatives, including his brother Homer, murdered by political opponents in the infamous Coushatta massacre of 1874. Portraying a separate attack, the author stunningly recreates how rifle fire from "the stranger in the green eye goggles" crippled Marshall Twitchell and left him for dead. It is widely recognized that Reconstruction was an era of highly partisan journalism, but the virulent invective that northern Democratic journalists directed at Twitchell after he was gunned down was notable nonetheless.

Tunnell makes excellent use of sources, including letters, memoirs, wartime and postwar military documents, congressional hearings, Freedmen's Bureau records, contemporary newspapers, various financial and credit reports, and family scrapbooks. Writing with verve, he evokes Louisiana's weather and geography. The individual personalities of members of Twitchell's family spring to life, as do their goals for reform and their economic designs and disappointments. Tunnell makes a strong case that, by advocating civil and political rights for African Americans, as well as being so influential economically, Twitchell and his family began to reshape Louisiana, provoking antagonists to threaten and then perpetrate acts of violence against them. In other words, Tunnell has written an effective biography: readers will get to know Twitchell and his world. Helpful maps, a good index, and several photographs and illustrations support Tunnell's rewarding text.

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JOHN C. RODRIGUE. *Reconstruction in the Cane Fields: From Slavery to Free Labor in Louisiana's Sugar Parishes, 1862-1880*. Baton Rouge: Louisiana State University Press. 2001. Pp. xvi, 224. Cloth \$49.95, paper \$19.95.

In this book, John C. Rodrigue undertakes to persuade readers that the former slaves of Louisiana's sugar parishes had unique opportunities to shape the economic and politics of their postemancipation world. Compounding the challenge, Rodrigue pins much of his analysis to a reconception of plantation gang work, archenemy of freedpeople in other crop regions. He argues that sugar workers derived power from centralized plantation routines and gang work. Rodrigue offers important information about the sugar region on the way to a conclusion that finally falls short of being fully persuasive.

He argues, first, that the realities of sugar production in Louisiana gave freedpeople powerful tools that they used to make steady marginal improvements in their circumstances. Sugar plantations did not break up into small, tenanted farms, because sugar processing could not be done piecemeal. Planters and freedpeople collaborated to maintain large centralized communities of laborers, working in gangs and collecting wages. Rodrigue argues that living in tightly connected groups enabled sugar workers to sustain job actions and organize politically more easily than their dispersed counterparts in other regions.

Louisiana's short growing season also required careful timing of planting, harvesting, and processing. Workers could and did readily threaten planters' tight schedules with small actions. As Rodrigue shows, workers used their leverage to drive wages up, secure additional pay for non-crop work, and resist planters' desire to hold wages back until the end of the year.

Moreover, Rodrigue argues, planters rarely unleashed retaliatory violence against workers. Planters feared "demoralizing" their workforce because they needed committed, energetic laborers every year during the rolling season. A successful rolling season, when cane was crushed and the syrup boiled and crystalized, required extraordinary investments of physical strength and management ability, as well as considerable intelligence about sugar's ornery chemistry. Poor productivity at the mill could negate the profitability of even the finest crop.

Finally, sugar planters supported the Republican Party's tariff policy, to protect themselves from Caribbean sugar. Their sympathies with the Republican Party created a divided planter class, making it somewhat easier for black Republicans to survive and operate actively in Louisiana. Rodrigue asserts that this combination of large worker communities, leverage on wage and job issues, and sustained access to political influence made Reconstruction in the sugar parishes uniquely promising for the freedpeople.

Rodrigue's analysis tests some of the settled assumptions of postbellum labor history against a less-studied crop system and shows how freedpeople participated actively and creatively in designing the postwar world. He also provides a fresh and interesting chapter on technological innovation in sugar production. But he is not able to establish that Reconstruction in the sugar parishes was distinct, let alone unique, except perhaps in some details of tactics and timing. As recent scholarship has made clear, every Reconstruction labor system offered freedpeople some degree of leverage, and freedpeople generally fought hard and creatively for their vision of free labor. And despite Rodrigue's repeated contention that the exigencies of sugar production forced planters to make concessions, his planters prove repeatedly willing to risk the loss of their crops and even their lands rather than compromise their claims to property and racial power.

The mixed political allegiances of Louisiana planters certainly helped freedpeople, but so did persistent Unionism and Republicanism in Upper South states and divisions between smallholders and planters across the piedmont. In Louisiana as elsewhere, freedpeople made use of whatever political opportunities came to hand. Louisiana's divisions no more guaranteed freedpeople their political rights than did any other political climate. And, as Rodrigue himself shows, especially in his final chapter on a massacre of resisting laborers in Thibodaux in 1887, freedpeople in the sugar lands, like those elsewhere, succeeded only as long as they avoided provoking the armed power of the state. In this, too, the sugar parishes look like crop regions elsewhere in the former Confederacy.

Rodrigue argues convincingly that black mobilization, resistance, and economic progress survived Redemption in the sugar parishes, but so did it also in cotton and tobacco-growing areas. The unrestricted violence and disfranchisement of the 1890s were far

more devastating to freedpeople's hopes than the 1870s had been.

Some of these similarities may have escaped Rodrigue's analysis because his sources on Reconstruction in the cotton and tobacco regions are remarkably old. Key comparisons to Louisiana are based on arguments made in the 1960s and 1970s, and few of his secondary materials on other crop regions postdate 1985. Few older analyses reckoned with the richness and focused self-development of freedpeople's communities around the South. This outdated frame of reference may have made Rodrigue's story look more distinct than it actually was.

Rodrigue persistently fails to engage with the voices of black sugar workers themselves, another casualty perhaps of his dated sources. Rodrigue's lens throughout is the trouble whites faced in securing, organizing, disciplining, and fighting with black workers. Far too often, the planters are permitted to speak for and explain their workers' motives. Though Rodrigue usually qualifies planter perspectives appropriately, freedpeople's voices should not remain almost silent in an exploration of their aspirations and achievements. More recent work on freedpeople might have suggested sources for the admittedly elusive perspectives of black workers, including freedwomen, who play virtually no role in this analysis at all.

Ultimately, Rodrigue offers an effective narrative of postwar adjustment in the sugar parishes, filling in our understanding of what happened there. But the book actually confirms the opposite of what Rodrigue wants to argue. Louisiana, despite the distinctive realities of sugar production, passed through Reconstruction, Redemption, and the rest without deviating in fundamental ways from the experiences of cotton or tobacco regions.

SHARON ANN HOLT

The Historical Society of Pennsylvania

DAN R. FROST. *Thinking Confederates: Academia and the Idea of Progress in the New South*. Knoxville: University of Tennessee Press. 2000. Pp. xiv, 207. \$27.00.

Resistance versus reform, cycles versus progress, Old South versus New South, classics versus applied sciences, Civil War generation versus their successors: these are the oppositions through which Dan R. Frost seeks to examine the idea of progress as it was developed in the South's postwar colleges for white males. Most of the presidents and professors of these institutions were former Confederate officers who took the positions out of a desire to maintain a status commensurate with their stature at a time when traditional opportunities in planting, politics, and the professions were few. Having witnessed the devastating disadvantages resulting from their region's reliance on agriculture, these men sought to introduce curricular reforms aimed at raising a new generation of men trained in the technology of the times who could set

the South on the road to prosperity. In doing so, they helped develop an ideology of progress that, Frost argues, both validates their status as intellectuals, undermining the conventional academic wisdom that views them as gentlemanly leaders hostile to change, and challenges the efforts of southern Progressive educators of the late nineteenth century to take sole credit for the "educational awakening" of the 1880s and 1890s.

The dominance of the North in science and industry caused the Union victory to appear inevitable to many educated men of the Confederate generation. The notion of inevitability had a number of welcome consequences. The stigma of slavery could be overlooked and the Lost Cause could be lauded, for noble soldiers had staved off certain defeat for four long years. Such an interpretation had the further advantage of not being an obstacle to imitating northern industrialization. Furthermore, the language of Anglo-Saxon superiority, shared by both sections of the country, could become the glue with which to repair nationalism and capitalize on reconciliation.

The issue initially underlying the development of a paradigm of progress was the struggle to institute a scientific education. Frost sets the stage with an examination of antebellum colleges and Jefferson's introduction of the elective system. The postwar leadership of the former Confederate officers, distressed by the deprivations they had experienced during the war despite the Confederacy's efforts to create and expand its industrial base, urged the introduction of courses in applied sciences like engineering. They failed to make substantial progress, however, for lack of resources, but they nevertheless instilled in their students the importance of applied science. When their students took over the leadership of higher education in the South, they thought the lack of previous success was evidence that the Confederate generation had not sought change. Yet, at the end of their day the South still lagged behind the North, and the New South remained as much a hope as an achievement.

As Frost points out, the struggle itself was built on the false premise that skilled students could overcome the lack of capitalization to build the New South. However, he does not examine this issue. Instead, his work focuses narrowly on the ideas of some of the more important college presidents and a few of the better known professors and is based primarily on their archival collections and those of their institutions, together with reports of their speeches in local newspapers. The reader cannot judge the extent of change in more than general terms, because there is no systematic discussion of the academic offerings of the institutions led by these men and the proportions of students enrolled in scientific courses compared to those taking the classical program. Nor is it possible to view their work in a larger context, because there is no systematic comparison to the curricula of comparable institutions in the rest of the nation. Frost does, however, compare the ideas of the two generations

of educators, demonstrating that the Progressives elaborated on the ideology through their views on women, blacks, and government regulation of industry. They inculcated this revised vision through courses in history, English language and literature, military drills, and physical labor, using the concepts of the Lost Cause, Anglo-Saxonism, and the New South to perpetuate their ideology of progress through applied science.

Scholars often seek to discover forerunners of movements in previous generations. Frost has taken this task one step further in the case of Confederate educators. He shows that they were central to the propagation of the idea that science and technology are the keys to progress—a belief that formed the basis of New South ideology.

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EDWARD J. CASHIN and GLENN T. ESKEW, editors. *Paternalism in a Southern City: Race, Religion, and Gender in Augusta, Georgia*. Athens: University of Georgia Press. 2001. Pp. xiii, 240. \$50.00.

Derived from a 1996 symposium sponsored by the Augusta State University Center for the Study of Georgia History, this collection of essays explores race, gender, class, and religious issues in the city of Augusta from the colonial era into the twentieth century. The contributors analyze these subjects through the filter of paternalism, though they do not allow that concept to control the narrative.

Paternalism is hardly a new theme in southern history, dating at least as far back as the work of U.B. Phillips. But paternalism is not often applied to an urban setting, and that is an important contribution of this volume. Editors Edward J. Cashin and Glenn T. Eskew view paternalism as a matter of negotiation between individuals and groups who have power and those who have much less. Within these unequal relationships, women, African Americans, and workers of different races and genders managed to carve out a measure of independence and even modest success. Issues of agency and power are increasingly prevalent subjects of inquiry in southern historiography. Though presented as a case study of one city, the essays collectively have significant import for southern history generally. Some of the contributors' ideas have appeared in books and articles published since 1996, but the collection is still relatively fresh and valuable.

Cashin's opening essay, "Paternalism in Augusta: The Impact of the Plantation Ethic upon an Urban Society," offers a historical overview of paternalism in Augusta within a broader regional concept, although he merely asserts its decline since the 1960s. Perhaps a better reading of the phenomenon in recent years is that it went underground but is still very much operative in some spheres of local life, especially with respect to religion and gender. Michele Gillespie, in "From Household to Market: Black and White

Women at Work in Augusta, 1790–1825,” demonstrates how the work opportunities for women emerging in a growing city moderated traditional paternalism in Augusta, albeit differently depending on class and color. In “Paternalism and Protest in Augusta’s Cotton Mills: What’s Gender Got to Do with It?” LeeAnn Whites imaginatively demonstrates how white male mill workers challenged paternalism and how their entry into the mill work force benefitted women workers.

Religion played a significant role in the development of Augusta, and Eskew’s contribution, “Paternalism among Augusta’s Methodists: Black, White, and Colored,” relates how paternalism provided the basis for interracial cooperation in the city’s religious community leading directly to the founding of several important black institutions in the late nineteenth century. But, unlike Julie Walsh’s essay later in the book, Eskew’s analysis does not clarify the motives behind such interracial cooperation. Was white support primarily a result of a religious commitment? Or, were there motives of social control, or simply a desire to maintain separate churches and institutions? Kent Anderson Leslie’s “No Middle Ground: Elite African Americans in Augusta and the Coming of Jim Crow,” resurrects several important African-American figures in Augusta’s past to demonstrate effectively how each responded differently to the racial restrictions of Jim Crow, ranging from migration and “passing” to Lucy Craft Laney’s aggressive support for black education.

Bobby J. Donaldson, though not a speaker at the symposium, offers a strong complement to Leslie’s piece in “Standing on a Volcano: The Leadership of William Jefferson White.” Donaldson’s portrait of a black leader who began his working career as a carpenter before the Civil War, and who became a prominent churchman, educator, and newspaper editor, provides a fascinating case study of how increasing racial restrictions in the late nineteenth century frustrated and nearly broke a talented individual. The concluding essay, Walsh’s “Rolling Religion down the Hill: Millworkers and Churches in Augusta,” offers a nuanced analysis of the mixed motives of wealthy whites in establishing mission churches in a mill neighborhood and how white workers reacted in various ways to these ministrations. Her depiction of two white ministers, the Reverend John Chipman and the Reverend J. S. Meynardie, and their social and labor activities are especially instructive of the mixed role of religious institutions in mill communities.

Although the essays are of uniformly high quality, the city of Augusta is, curiously, often missing from the volume. With the exception of Gillespie’s contribution, the process of urban development plays little role in the narratives. We do not learn much about the transformation of the city, the changing economic base, demography, and political context over time, and how those changes affected the issues the contributors address. And that is unfortunate, because the possibilities offered by urbanization—the growing economic

diversity, in-migration of blacks and whites from rural areas, expanding trade patterns, and the growth of local government—had direct bearing on race, class, gender, and religious issues. The tightening strictures of Jim Crow, although obviously a region-wide phenomenon, had particular significance for a developing urban society. The southern city, after all, became the prime venue where racial separation played out its most tragic results, both in dreams dashed and lives lost. And it is not clear how or if paternalism functioned differently in the urban setting from its workings in the rural South. Did “urban” make a difference?

Still, this anthology extends its reach beyond the study of one city and has significance for a variety of southern history topics. Collectively, the essays demonstrate the importance of Old South ideals in shaping New South social and political relations. The contributors also illuminate the role of agency within the confines of paternalism without implying that such agency in any way lessened the restrictiveness of paternal power, whether exercised over working-class whites, women, or African Americans.

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LARRY EUGENE RIVERS and CANTER BROWN, JR. *Laborers in the Vineyard of the Lord: The Beginnings of the AME Church in Florida, 1865–1895*. (The History of African-American Religions.) Gainesville: University Press of Florida. 2001. Pp. xx, 244. \$34.95.

Larry Eugene Rivers and Canter Brown, Jr., have responded to the challenge of providing a close examination of African-American churches during the second half of the nineteenth century, a highly critical period of adjustment from slavery to freedom. They lay out the framework of the African Methodist Episcopal (AME) Church in Florida from its founding in 1865 to 1895. The latter year seems less directly pertinent to the AME Church than the first. In May 1865, AME officials dispatched the first denominational missionaries into Florida. But the authors chose to end their book in 1895 because of several occurrences, including Frederick Douglass’s death, Booker T. Washington’s famous “Atlanta Compromise” speech, and devastating freezes in Florida that altered political, social, and economic circumstances for African Americans. This book is the first in a new series called “The History of African-American Religions,” edited by Stephen W. Angell and Anthony Pinn. The editors intend to “further historical investigations into the varieties of African-American religions and to encourage the development of new and expanded paradigms, methodologies, and themes” (p. xi) in the history of African-American religious experiences.

In this volume, the authors give us a lean, crisply written narrative of church-building beginning in the closing stages of the Civil War. Although several

preachers had congregations primed to unite with the AME Church when its missionaries entered the state, two individuals led the way in organizing the denomination at a time when Key West was the largest town but more than fifty percent of the state's African-American population resided in northern and central counties. William G. Stewart, a former Georgia slave, was a preacher who bore a "simple faith" (p. 23). Charles H. Pearce, free-born in Maryland, was an ordained AME minister with missionary experience in New England and Canada. These contrasting clergymen represented the basic sources of the church's ministerial personnel; they also illustrate the tension that developed within the AME Church, especially when prelates like Bishop Daniel Alexander Payne pushed vigorously for an educated ministry. Payne was also instrumental in directing the AME Church away from political activity. Pearce, who became embroiled in Reconstruction politics and served in the state's constitutional convention and legislature, exemplified the type of involvement that Payne believed was detrimental to the AME Church and its principal responsibility of spiritual uplift.

Long before the disfranchisement of African Americans, the AME Church had largely withdrawn from state and local politics. Rivers and Brown tell this story well. They also provide sketches of many of the denomination's ministers, but, for the most part, they note the organization of congregations, the formation of annual conferences, and efforts to establish and maintain ministerial training schools. They also offer some explanation for the slow and unsteady growth in AME membership.

The authors shy away from rich texturing, especially in terms of the experiences of African Americans within the AME denomination. For example, while Rivers and Brown point out that Florida was the scene of an uncommon amount of antiblack violence, they provide us with little sense of how the AME Church dealt with such terrorism. Nor do they describe the people who joined AME churches, what they believed, how they worshipped, or the material difference the church made in their daily lives. One way to enhance our understanding of the AME Church is to compare it with other churches. Potential converts had choices, but we do not learn much about why they chose the AME Church—or why they did not.

Rivers and Brown fall somewhat short of presenting new paradigms or methodologies. Their story is a solid though traditional administrative history. It is also a mostly gendered one about the successes and failures of male ministers. And to tell it, they have reached largely into conventional sources: church and public records, newspapers (the local Florida and national presses), as well as a handful of manuscript collections. They have also drawn from a growing body of published secondary works.

This is a valuable book, however, one that makes important contributions to our understanding of African-American religious experiences. It is the most

thorough examination of African Methodism in Florida, one that is easy to read and useful to specialists. The names of people and congregations in the book will assist further research. Rivers and Brown add needed depth to the expanding literature on African-American church history.

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FERENC MORTON SZASZ. *Religion in the Modern American West*. (The Modern American West.) Tucson: University of Arizona Press. 2000. Pp. xviii, 249. \$35.00.

Ferenc Morton Szasz's pioneering historical overview of religion in the modern (twentieth century) American West (from the 100th meridian to the Pacific Coast) challenges the adequacy of conventional accounts of the nation's religious history that are told from exclusively eastern perspectives. With the West included, the national story is more rich and complex than we have known. Long-accepted generalizations now must be altered. First, the Spanish Catholic and Russian Orthodox colonial heritage qualifies assumptions about essentially Anglo-Protestant foundations of American religious history. Second, by the end of the nineteenth century, migrations and settlements in western regions had come from all directions and represented ethnic cultures of the world, which fostered an unusually pluralistic religious life in the West. Third, therefore, no hegemonic Protestant mainstream ever developed in the West overall, while local congregations of diverse Catholic, Protestant, Jewish, and other religious traditions flourished under what Szasz calls "a largely secular cultural umbrella" (p. 194). Fourth, add to this the enormous significance of the development of a new indigenous American world religion from within the Mormon empire at the crossroads of the West, the continuous unfolding of Asian world religions (especially varieties of Buddhism, Islam, and Eastern Orthodox Christianity), and the flourishing of Native American spiritualities, and we have a new perspective on the nation's religious history.

Until the appearance of this book, nearly two decades of western American religious historiography had produced bits and pieces without a coherent narrative taking account of both unifying themes and distinctive subregional stories. Szasz's narrative succeeds because it never loses sight of familiar developments in national religious life and because it follows standard periodization of American religious history while allowing regional events, institutions, and personalities to provide the substance of the story. It is noteworthy that emerging new mass media (radio, film, TV) contributed to some western religious personalities assuming national popular fame, such as Aimee Semple McPherson, Oral Roberts, Robert Schuller, and Alan Watts. Others achieved reputations far beyond their regional boundaries, such as John Muir,

Rabbi William Friedman, Cardinal James Francis McIntyre, Bishop James A. Pike, Reverend Cecil Williams, Archbishop Raymond G. Hunthausen, and the writer Kathleen Norris. But the author's larger contribution may be in introducing religious leaders previously limited to more local influence who will merit recognition in new accounts of American religious history. Because these regional personalities represent many religious traditions and movements, their emergence from historical obscurity contributes to a significant development in American religious historiography. The strength of Szasz's account is his consistent attention to religious pluralism throughout the West, while never losing sight of the widespread relative strength of culturally diverse Roman Catholic parishes and the ever-increasing strength and influence of Mormon culture.

Overall, Szasz effectively traces and analyzes the development of American religious phenomena as they appear in western contexts. Fundamentalist-modernist conflicts that raged in the eastern states during the 1920s, for example, did not become much of a factor in most of the West until a half-century later, when creationism versus evolution became a widespread religiocultural conflict in western life. Southern Protestant migrations during the Great Depression and after help to account for these shifting patterns of national religious life, which include the centering of much of late twentieth-century resurgent neo-evangelicalism in the West. But the existence of many western contexts, where the impulses and constraints of religious traditions have been weak in public culture, has also facilitated such vigorous new religious movements as Scientology, the Unification Church, the Rajneeshpuram community in Eastern Oregon, and even the disaster associated with the People's Temple moving from San Francisco to Guyana.

At some points in Szasz's account, topics and periods do not fully mesh. Oral Roberts, for example, who rose to national popularity during the 1960s and 1970s, is treated prematurely in the chapter on the 1920s and 1930s, when Pentecostalism became a religious subculture in parts of the West. Conversely, African-American church life makes a late appearance in the section on the 1960s to the present, whereas the great migration of African Americans to Pacific Coast wartime industries during the 1940s presents a more engaging historical moment for introducing this complex tradition into the narrative.

Szasz admits that space limitations forced him to make critical selections. However, his argument for a discreet regional western religious history could have been strengthened had he not made some choices of omission. For example, by excluding Alaska we miss out on the enormously significant Presbyterian home mission leader Sheldon Jackson, who connected the Rocky Mountain West to the North Pacific. Likewise we lose a continuous pluralistic religious impulse that has tied the North American Pacific region from Alaska to Mexico and engaged world events by the

omission of the successive twentieth-century waves of Russian immigration. Most notably absent, however, are the railroad chapel cars that carried Episcopal, Baptist, and Roman Catholic missionaries throughout the western states from the 1890s through the 1930s and stimulated religious life everywhere they paused along the way.

Even to think of topics neglected, however, is to pay tribute to Szasz's pioneering accomplishment. Thoroughly researched from archival and printed sources, and taking account of the published literature on aspects of the subject, this book now becomes the landmark account that historians of religion in the modern American West must turn to first.

ELDON G. ERNST

Graduate Theological Union

ROBERT M. FOGELSON. *Downtown: Its Rise and Fall, 1880–1950*. New Haven: Yale University Press. 2001. Pp. x, 492. \$35.00.

Robert M. Fogelson opened his distinguished career as an urban historian with *The Fragmented Metropolis: Los Angeles, 1850–1930* (1967), a story of decentralization and suburbanization. A third of a century later, he has come full circle with this very good book about the downtowns of big cities such as New York, Boston, Philadelphia, Chicago, St. Louis, and—yes—Los Angeles.

Fogelson's book is a history of public policy and planning drawn from the published record. The author bases his analysis on the writings of late nineteenth and early twentieth-century business leaders, real estate professionals, highway engineers, architects, urban reformers, and city politicians. The footnotes bristle with references to metropolitan newspapers, local planning reports, and trade journals such as *Electric Railway Journal*, *Brickbuilder*, *National Municipal Review*, and *Proceedings of the American Society of Civil Engineers*. He supplements this dense reading of contemporary discourse with examples drawn from the large secondary literature on urban growth and planning. At the same time, his downtown perspective offers new ways to think about such standard urban history topics as transportation policy, land use planning, the rise of the skyscraper, housing reform, and urban redevelopment.

The result is a history of decision makers: how they thought downtown functioned, what they thought it needed, and how and why they so often disagreed in their diagnosis and prescriptions. In the author's own words, "the book is about how Americans shaped (or tried to shape) downtown, not about how they felt downtown. Hence . . . it deals mostly with those with the most power" (p. 7). Fogelson thus provides a framework that other historians can utilize in exploring related topics such as the evolution of the white-collar workforce, the ceremonial functions and spaces of urban centers, and the role of downtown businesses and places in acculturating new Americans.

The book starts in the 1880s and 1890s with the

emergence of downtown as *the* concentrated business district of American cities, a pattern that contrasted with the more dispersed business districts of European cities. By the 1920s, downtown was reconceptualized as the *central* business district, surrounded by growing but still subordinate business clusters. By 1950, even downtown boosters feared that its future was to be "just another business district" (the title of the penultimate chapter). The book thus ends with the origins of the federally financed urban renewal program in Title I of the Housing Act of 1949, the point when other accounts of downtown planning and redevelopment efforts often begin.

Fogelson has two ongoing themes. One is the importance of language in shaping policy assumptions. He has great fun in detailing the metaphors through which city leaders understood their business districts and justified their promotion. Wheels had hubs, bodies had hearts, and cities had downtowns. Hearts functioned best if arteries were unclogged; so, too, did downtown require that traffic congestion be reduced and access be made fast and easy. Organisms could also become diseased, with cancerous blight strangling the central business district and outlying shopping districts sapping its strength like parasites.

The second theme is the importance of spatial politics. Downtown was always contested, even at the height of its prominence. Merchants and property owners on one downtown street bickered with those on another street over the location of mass transit terminals. People who owned property on the most valuable core blocks battled those who owned land on the edge of the central business district. Core interests as a group fought with residents and business people in uptown districts and then in independent suburbs. The battles revolved around public transit investments, height limitations on buildings, zoning, and slum clearance. The common question was whether it was more efficient and beneficial for the metropolitan community as a whole to promote concentration of people and activities (for example, by constructing radial subways and radial highways) or to spread them out (for example, by capping the height of skyscrapers). And, if the latter, was the goal a less intense but still centralized business district, or a network of smaller districts?

Fogelson has filled a gap in American urban history with a carefully wrought book that specialists on urban development will want on their shelves. He has also written a cautionary tale that speaks to contemporary concerns as well as historical questions. A number of American downtowns seem to be recovering from their low points in the 1960s or 1970s, thanks to new cultural institutions, growing tourism, and a boom in middle-class housing. But readers who are familiar with debates over new subway and light rail systems and with the most recent generation of downtown plans will realize that we may simply have dressed up the old debates with new numbers and graphics. If we follow Fogelson's implicit advice, we will want to examine the

metaphors that drive twenty-first century policy and planning with as much care as his book gives to those of previous generations.

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SVEN BECKERT. *The Monied Metropolis: New York City and the Consolidation of the American Bourgeoisie, 1850–1896*. New York: Cambridge University Press. 2001. Pp. xvii, 492. \$34.95.

This is a study of New York's upper class in the second half of the nineteenth century. This metropolitan establishment was at first commercially and then industrially and financially based and expanded to exert hegemony over politics, philanthropy, and culture. At the turn of the century, by dint of its economic and political power, it wielded virtually national leadership. Sven Beckert explores the consolidation, largely through control of capital, of New York's upper class, and, given its commanding position in this quintessentially capitalist and bourgeois nation, seeks to illuminate the emergence of modern America through tracing the trajectory of this enclave.

In a historiographical era predominantly devoted to microtopicality, this is a bold and welcome venture. Beckert also departs from the current trend in historiography by returning to structural history (the making of an upper class) instead of featuring cultural analysis. As an exploration in political economy, his book is a methodological reversion to an earlier mode of scholarship. Similarly its concern with the rich and the powerful, rather than the marginalized and their struggle for agency, harks back to older historical concerns. Beckert's sympathies, however, which lie with the proletarianized workers, are more current.

Beckert deftly traces the consolidation of the enclave, properly labeled a bourgeois archetype. Elites in a nation lacking feudal traditions and almost universally proclaiming middle-class values would epitomize the bourgeois outlook. He exhaustively traces its economic rise and cohesion and superbly delineates the interaction between its economic interests and its political defense of these interests. The discussions of laissez-faire and antilabor politics and the political interaction between upper-class New Yorkers and the South are excellent, and other examples of exceptional analysis and research could easily be summoned. As a study in political economy, there is no superior rendering of Gotham's *haute bourgeoisie*.

Beckert falters only at the most elevated levels of historical endeavor. His book is a major contribution to an important topic (urban upper classes), but it does not reconfigure the field (elite studies). The interpretation of upper-class formation, consolidation, and cohesion is derivative. The notion that urban elites first established economic hegemony, added political functions to fortify their business base, and subsequently acquired positions of social and cultural leadership—

thus expanding from an economic elite into an upper class—was advanced twenty years ago.

Beckert might have made some theoretical contribution by comparing other urban elites. Charleston, South Carolina, had an elite mostly in foreign trade and agriculture, Chicago chiefly in industry, banking, and retail trade, and Boston in foreign commerce, industry, and banking. Did the establishments in these cities follow the same trajectory as in New York? If not, in what ways did they differ and what caused these differences? An attempt at comparative typology may have yielded insights into the rise and decline, the cohesion and continuity of elites both on the conceptual level and with respect to New York and even on the bourgeois and patrician components in American upper classes. While enclaves were essentially bourgeois, they also had patrician elements: conspicuous display, ascriptive features such as family ties and endogamous marriage, and the assumption of setting standards of etiquette and style in culture and society. Beckert pays some attention to these dimensions of elite behavior but neglects to mention that these are aristocratic rather than bourgeois in origin and nature.

Beckert is more effective on his own turf than in the realms of the conceptual and comparative. Nevertheless, several contentions about the New York establishment present problems. According to him, the antebellum mercantile elite was more egalitarian and democratic than was its industrial-financial successor. Perhaps this proposition is correct, but it is not rigorously tested. Deferential and dynastic politics did not end with the revolution, and well-born merchants and their relatives showed up with great frequency in high government positions and, for that matter, were similarly situated in banking and other businesses and in leading charitable and cultural institutions. Related to the alleged decline of democracy is Beckert's assertion that wealth concentration in the industrial-financial multimillionaires was far greater than in their mercantile predecessors. Again, the response is perhaps but not proven. Did these moguls own a larger share of the total local or national assets than did their predecessors? Data on the distribution of wealth in the nineteenth century is available but not systematically examined.

A final problematic assertion is that the mercantile and industrial enclaves merged and thus a circulation of elites did not take place in nineteenth-century New York. Here, too, Beckert's arguments and assertions are impressionistic rather than systematic. He cites intermarriages between these two groups but does not indicate what percent of the marriages of the industrial clans were to mercantile families. Equally impressionistic counter-examples, such as rejections of potential spouses from the older elite for unions with European nobles, could be cited. There was, in fact, considerable hostility between the parvenus (the establishment discussed in this book) and the Knickerbocker-mercantile patriciate (Edith Wharton's set). They lived in different neighborhoods and often snubbed each other. But

even if they had not, and despite Beckert's correct claim that they sometimes served on the same corporate, charitable, and cultural boards, he has not proved that the older leadership enclave was not displaced. It is one thing to be a trustee; it is another to run the enterprise. Scions from Knickerbocker and mercantile families were on the board of the Metropolitan Museum of Art, but J. P. Morgan was the controlling influence. Here, too, comparison with other elites would have helped. The industrial enclave of the Boston upper class better integrated with the Brahmin mercantile patriciate than did the industrialists and financiers of late nineteenth century New York with their predecessors.

Reviews often emphasize the critical, and my reservations should not detract from appreciating the contributions of Beckert's book to elite studies, urban history and the field of political economy.

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XINYANG WANG. *Surviving the City: The Chinese Immigrant Experience in New York City, 1890–1970*. (Pacific Formations: Global Relations in Asian and Pacific Perspectives.) Lanham, Md.: Rowman and Littlefield. 2001. Pp. x, 157. Cloth \$59.00, paper \$22.95.

Much of the scholarship on the Chinese immigrant experience in the United States has focused on immigration in the mid to late nineteenth century and the subsequent anti-Chinese movement, which led to passage of the Chinese Exclusion Act in 1882. Due to the racial discrimination that was central to this movement and the amount of legislation that was passed against the Chinese on the federal, state, and local levels, Chinese immigrants are often portrayed as victims of racism with little recourse to shape their lives in America. Many previous scholars have attributed their ability to survive this harsh treatment to their ethnic and cultural resilience or their docility in the face of discrimination. In this work, Xinyang Wang attempts to offer a new understanding of the Chinese immigrant experience in New York City by focusing on the role of economic decision making, emphasizing that economic survival and racial discrimination must be analyzed together in order to gain a more complete understanding of why and how Chinese immigrants made the decisions they did. In addition, Wang seeks to broaden our understanding of Chinese immigrants by comparing them to Italian immigrants of the same time period, along with occasional comparisons to the Chinese in San Francisco and other cities.

Wang's primary interest is to stress that the Chinese immigrant experience was more complex than simply their response to racism. He maintains that "If racial hostility had been the only factor affecting Chinese adjustment in the United States, then the immigrants should have continued to return to China," and he asks, "why did they suddenly reverse their 'sojourner'

mentality in the early 1950s and decide to stay in America?" (p. 6). His answer to these questions lies in his position that "the need for economic survival was even more crucial than avoiding racial hostilities . . . The New York Chinese were obviously rational actors who compared the different options available to them and made their choices primarily for their own survival" (p. 131). Very few would argue with this general conclusion. Chinese immigrants certainly understood the choices before them and they made whatever choices they could which best suited their circumstances. Some returned to China, some moved around in search of better opportunities, and others made the best of the situations in which they found themselves. In this sense, Chinese immigrants were similar to others who immigrated in search of economic opportunity.

Although Wang should be commended for attempting to broaden our understanding of Chinese-American history, and his comparison of Chinese and Italian immigrants is an approach that more immigration historians should consider, this slim volume does not explore the issues sufficiently to alter our understanding of the Chinese presence in New York or other parts of the country. His focus on only the immigrant generation is understandable, but by doing so, he reiterates some of the longstanding narratives of the immigrant generation that ultimately serve to undermine his attempt to dispel long-held stereotypical views of Chinese immigrants. Most significantly, he depicts Chinese immigrants, regardless of their date of entry, as "bachelors" with a "sojourning mentality" bent on returning to China. It is now well known among historians of Chinese Americans that many of the immigrants were not bachelors at all but had families in China. Their role was to labor in America and send home remittances in order to support those families. This is an important explanation for why immigrant men did not return to China in spite of American racism. They could still earn more in the United States than in China, thus the family was more financially secure than had the laboring husband/father returned home. After 1950, due to the Chinese Communist victory, Chinese-American remittances were needed more than ever. Furthermore, many Chinese immigrants started families in the United States, with Chinese immigrant women, Chinese Americans, and non-Chinese women. Once these families were established, the desire to return to China lessened. In order to understand better the Chinese immigrant and Chinese-American experience, the interaction across generations must be considered. To study only immigrants between the years 1890 and 1970 is to neglect important developments that had vital ramifications on how Chinese immigrants and their families saw themselves in relation to China and the United States.

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ERIC SANDWEISS. *St. Louis: The Evolution of an American Urban Landscape*. (Critical Perspectives on the Past.) Philadelphia: Temple University Press. 2001. Pp. xiii, 282. Cloth \$74.50, paper \$24.95.

Like so many urban districts that blossomed during the nineteenth century in industrial cities, the South Side of St. Louis, with its neat brick rowhouses, tiny lawns, back alleys, and corner taverns, evinces the romantic ideal of neighborhood as tight-knit community. But as Eric Sandweiss deftly argues, the process of linking community to place in St. Louis was both contested and fluid. In bricks, mortar, and asphalt, the South Side neighborhoods articulated a struggle to balance individual liberty and the common good. During the most active period of construction, the years between 1850 and 1910, that struggle found expression in the tension between two opposing physical expressions of community: the "fenced-off corners" of the home and street and the "wider setting" of the larger urban environs.

The debate wasn't always framed that way. In the first half of the nineteenth century, the conflict between local interests and broader civic goals was muted by the fact that the people who developed private land on the South Side and the people who spoke for the community at large in an official governing capacity were one and the same. A handful of elites furthered their own interests, and in their minds those of the city, by placing responsibility for urban development almost entirely in private hands. Even as city government became more actively involved in determining the contours of urban development, selling off lots in the St. Louis Common and ordering the improvement of certain streets, the overarching model of the city as a field for speculative investment imposed a unity upon all land within the city limits.

After 1850, an influx of immigrant laborers and the dispersal of decision-making power across a wider and more localized cast of builders and consumers enabled South St. Louisans to begin fencing off corners from the wider urban setting. Still, the road to a neighborhood-based brand of community identity was neither straightforward nor predictable. At first, families of common ethnic heritage found refuge from the larger city that intruded onto their streets in the shared and sheltered domestic spaces of the porch and back yard. Over time, as interior domestic spaces became more sharply delineated and residential populations grew more ethnically diverse, neighborly interaction moved to the public sidewalks, where large-scale developers were able to guarantee safety and uniformity through comprehensive deed restrictions. The most powerful force impelling a sense of local identity in South St. Louis, however, was the city's refusal to pave streets there. Ironically, steadfast adherence to the larger civic goal of fiscal prudence produced an uneven streetscape that fragmented the South Side into a patchwork of isolated villages.

It was in reaction to the proliferation of fenced-off

neighborhoods and the brand of ward-based politics to which they gave rise that elite reformers and planners proposed a vigorous program of physical reconstruction in which power and civic values would flow from downtown to all outlying districts. The dispersal of neighborhood civic centers, an integrated street system, and comprehensive zoning would reconnect the parts to the whole in the name of the greater public good. Southsiders resisted this centrally controlled urban order, repeatedly voting against bond issues that promised them few benefits. In the end, however, continued neglect on the part of professional planners spared South St. Louisans the massive disruptions associated with postwar renewal projects.

The meticulous detail in which Sandweiss charts the physical transformation of the South St. Louis landscape provides urban historians with a fresh model for researching and conceptualizing vernacular urban landscapes. By confining the analysis to one section of the city, and at times to more limited clusters of blocks, Sandweiss conveys the story of urban change in all its rich and messy details. Painstaking research into property deed transactions, city ordinances, and street paving contracts reveals landscape change as the product of negotiation among a wide cast of both public and private actors including builders, lenders, consumers, regulators, and planners. More impressively, Sandweiss demonstrates how architecture and infrastructure mediate between the complex social structures recorded in census manuscripts and idealized visions of the city represented in maps and planning reports. Finally, the book makes a powerful statement about the capacity of the built landscape to shape history. In addition to filling a void in the scholarship of one of America's most important industrial cities, it will make an important contribution to the larger field of urban history.

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THOMAS M. SPENCER. *The St. Louis Veiled Prophet Celebration: Power on Parade, 1877-1995*. Columbia: University of Missouri Press. 2000. Pp. xv, 204. \$29.95.

In the winter of 1972, civil rights advocates in St. Louis, Missouri, took aim at a rather unlikely target. The Veiled Prophet ball—the Midwest's oldest and most public debutante ball—had long given St. Louis's prominent families an opportunity to show off in each other's company. Given the city's grave socioeconomic problems—only six months before, St. Louis spectacularly demolished the infamous Pruitt-Igoe project, laying waste to a group of thirty-three public housing apartment blocks that had come to symbolize the city's failure to house its poor—what could be more trivial? But to the Action Committee to Improve Opportunities for Negroes (ACTION), social injustice and the ball were dialectically related. When ACTION disrupted the December ball by shouting “Down with the

VP,” scattering leaflets, and, most dramatically, clamoring on stage and removing the crown and veil that adorned the pageant's king, it sought to make that connection explicit.

That seemingly innocuous display events like the Veiled Prophet ball often serve the interests of those in power was not lost on civil rights protestors of the early 1970s, and it should not be surprising that historians have begun to investigate them as sites of cultural hegemony and resistance. Most studies examine the years surrounding a tradition's invention or, perhaps, make general comparisons about, say, pageants in Victorian England or parades in nineteenth-century Philadelphia. Few, however, trace the historical evolution of a display event over an extended period. Thomas M. Spencer attempts such a project and largely succeeds.

Like so many elite-orchestrated display events, the Veiled Prophet celebration was created in response to political economic crisis—in this case, the St. Louis general strike of 1877. Spencer details how prominent local businessmen paraded in the following year as a way to gain symbolic and physical control of the city's streets. With its namesake dressed like a hooded Klansman, replete with white robe, pointed masked hood, and carrying pistol and shotgun, the earliest Veiled Prophet parades displayed the St. Louis elites' power over the fractious working classes and, simultaneously, legitimated their position atop the city's social hierarchy. The accompanying debutante balls were not meant to be seen by “the masses,” but, instead, demonstrated their participants' social status in a much more controlled atmosphere.

Spencer shows how these two forms of cultural display changed through time and how they were always in dialogue with changes in the city itself. The historical and educational elements that were part of the nineteenth-century parade (the Klan motif was dropped early for reasons that are unclear) gave rise to the commercialized leisure that dominate public celebration today. The climax of the celebration—in St. Louis and in Spencer's retelling—occurred in the early 1970s with the multifaceted protests that began to unhinge it. Although the event continued after organizers dubbed it the “VP Fair,” by the 1990s it had lost much of its historic resonance and became just another amusement. The VP Fair, Spencer laments, devolved into “a cultural vehicle that is spent and largely meaningless” (p. 170).

Such nostalgia, however unwitting, leads the author to some unwarranted conclusions: “The answer of elites to the innate problems of conducting civic instruction in a diverse twentieth-century urban society has been to give up on presenting history to the public” (p. 170). This is a fallacious argument for several reasons. The self-serving nature of St. Louis elites' presentation of “history” during the Veiled Prophet ball makes one wonder if we are not better off without parades that “teach working class spectators about genteel middle-class culture and values” (p. 41).

Should we really mourn the loss of the sham “history” that positioned “savages” against “civilization”? While Spencer is correct to note the division between “academics” and “ordinary people” in understanding the past, overwhelming evidence—the popularity of historic sites, museums, novels, films, documentaries, the extensive movement to designate and preserve historic buildings, and the rise of genealogy—suggests that popular interest in the past has never been higher. As David Lowenthal has noted recently, the heritage crusade is on the rise the world over; it is just that the forms in which the past is popularly articulated, like the Veiled Prophet celebration itself, have changed.

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MARGUERITE S. SHAFFER. *See America First: Tourism and National Identity, 1880–1940*. Washington, D.C.: Smithsonian Institution. 2001. Pp. 429. Cloth \$55.00, paper \$18.95.

In this study, Marguerite S. Shaffer argues that domestic tourism had an important influence over American national identity. Among other things, the growth of the tourist industry helped to reconcile some increasingly apparent contradictions in American life, particularly the conflict between the nation’s romantic and mythological ideals of nature, democracy, and liberty and the growing reality of an urban and industrial nation-state. Furthermore, Shaffer claims that tourism fostered an ideal of “mobile citizenship that redefined political rights in consumer terms, celebrating seeing over speaking, purchasing over voting, and traveling over participating,” and in the process eroded the nation’s civic culture (p. 6). While her narrative ably demonstrates the growing importance of domestic tourism to American culture, the relationship between tourism and the demise of civic culture is somewhat more elusive.

Shaffer begins in the aftermath of the Civil War. As the nation turned its gaze toward the west, the growing network of railroads made possible not just industrial development but also an increasingly sophisticated and elegant world of train travel. One byproduct of this new mode of transportation was an increased interest among tourists in touring *across* America rather than the more traditional model of resort vacationing. Prior to the war, Shaffer argues, tourism had been primarily regional in cast, while in the latter half of the century it moved toward a more mobile ideal.

In part, this change was accelerated by the creation of the national park system, which pulled tourists west. Shaffer carefully documents the growing role of railroad corporations, and later automobile and highway promoters, in publicizing the parks for tourists across the country. Jay Cooke’s role in creating Yellowstone National Park as a means to promote the Northern Pacific Railroad just after the Civil War set a precedent that would be followed in later years, first by the

Atchison, Topeka, and Santa Fe Railroad in the Southwest, and then by the Great Northern Railroad’s promotion of Glacier National Park.

The power of the railroad corporations over western tourism peaked at the turn of the century but thereafter was rather rapidly eclipsed by auto companies, good roads organizations, and auto clubs. As Shaffer’s research illustrates, all of these organizations quickly recognized the centrality of a strong tourist infrastructure in the West. But like the era that preceded it, here, too, the experience and sponsorship of western tourism continued to be dominated by elites. In the automobile era, those elites able to afford automobiles—or profit from their manufacture—often pursued goals such as an interstate highway system at the expense of rural constituencies that were less eager to take on intense tax burdens for programs that they found unnecessary. And by creating a system of roads that highlighted historical contributions and events, the interstate highway movement helped a portion of the public to see their tourist adventures as civic contributions.

These examples of “corporate dominion” over western tourism coincided with—and in some instances benefitted from—the creation of the See America First campaign. Founded in 1906 by a small group of western boosters, the group used patriotic and nationalistic rhetoric to encourage American tourists to look westward rather than across the Atlantic. Such a decision, they argued, would be as much civic as commercial, teaching patriotism through tourism. The campaign had limited success in its early years but was aided immensely by the outbreak of war in Europe, which effectively closed the continent to American tourists.

One of the subthemes of this study is the dynamic relationship between geography and nationalism. To the community of boosters that propagated the growth of western tourism, geography itself became a way to learn love for America as parks became places to inculcate values of citizenship and national devotion. To Shaffer, the new rhetoric incorporated an element of territoriality into American citizenship. Here a comparison between this new rhetoric and the older language of Manifest Destiny might have offered more context for the history of this intellectual change and the connection between ideas of nation, geography, and identity.

In the last chapter, Shaffer frames tourism primarily in terms of consumption in order to stress how it consistently drew Americans away from civic consciousness. In her words, “Participatory democracy became a more passive, individualized experience acted out along an extended axis over mobility . . . The implications are that only those who could afford to have access to the tourist landscape . . . could participate in this ritual of citizenship and validate their identity as Americans” (p. 309). By Shaffer’s own admission, the tourist experience varied widely, and so the reader is left hoping for a more sustained devel-

opment of this argument. Her speculation here does, however, offer intriguing possibilities for our understanding of the relationship among leisure, consumption, and politics.

SUSAN SCHULTEN
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JAMES E. SNEAD. *Ruins and Rivals: The Making of Southwest Archaeology*. Tucson: University of Arizona Press. 2001. Pp. xxvi, 226. \$35.00.

The southwest was not always the Southwest. Before a big, largely empty, arid spot on the map could be transformed into a mystical place of ancient pueblos and mysterious "cliff dwellers," it had to be imagined as such—created, constructed, invented, and presented to a public eager to consume it.

How the southwest got to be the Southwest is the story James E. Snead tells us in his fun and readable book. That process, which began late in the nineteenth century and continues apace today, had many participants, including cultural entrepreneurs, antiquities collectors and dealers, antimodern spiritual seekers, railroad tourism promoters, and local economic development boosters. Snead focuses his attention on archaeologists and on their role in creating chronologies and cultural pedigrees from which the Southwest could draw its historical and social identity.

On one level, Snead has organized his book analytically around a set of tensions. Among them are the conflicts between archaeologists and institutions in the East and their western counterparts; the suspicion with which increasingly professionalized archaeologists viewed those they considered "amateurs" (this latter being largely a subset of the former, because archaeologists got their professional credentials in the East: those from the West were *ipso facto* amateurs); soul searching within the field of archaeology itself as many wondered what its relevance was and whether it belonged under the larger disciplinary umbrella of anthropology; frictions resulting from what Snead calls "the dichotomy between utilitarian science and humanistic interpretation" (p. 29); and the competitions between research and public education within the museums that shaped southwestern archaeology.

To tell this story, Snead has focused largely on the work sponsored by the American Museum of Natural History and that which was generated by local groups and patrons in the region itself. He begins in the 1890s with the uneasy collaborations between East Coast "museum men" and western "relic hunters," the spectacularly popular displays of Southwest archaeology at the 1893 World's Columbian Exposition, and the place of the Southwest in the emerging institutional framework of the American Museum, where Frederic Ward Putnam intended to make southwestern archaeology central to its department of anthropology.

In chapter two, Snead next takes us along with the Hyde Exploring Expedition in the 1890s and describes how it was embroiled in these very tensions. Mean-

while, local boosters, like Charles Fletcher Lummis, Edgar Lee Hewett, and Frank Springer, were busy developing what Snead calls in chapter three "the western idea," in which relics and ruins would play a central part. Recognizing the importance of building institutions to compete with New York, Cambridge, and Washington, D.C., these men were instrumental in establishing local archaeological societies as well as the Museum of New Mexico, and in locating the School of American Archaeology in Santa Fe.

From the American Museum came, next, the Huntington Southwest Survey, rising from the ashes of the Hyde Expedition that had collapsed in 1909. For Snead, one of the primary significances of the survey was its rigorous, and largely successful, efforts to create a workable chronology for southwestern archaeology. In so doing, it provided "the means to tackle large-scale problems of population movement and the organization of societies. In this way, an archaeology of relevance to anthropology could be established" (p. 122).

Snead brings us into the 1920s and a period where he sees a southwestern synthesis growing out of the various clashing theses and antitheses he has chronicled. This "program" for the Southwest resulted from the combination of the American Museum's "chronological archaeology" and the "heritage-centered archaeology of the local societies" (p. xxvi), and it lasted through much of the twentieth century. Indeed, as he points out, the Southwest is still the site of people searching for meaning in the past, either through science or new age pilgrimage.

Snead hangs his analytical hat on what he identifies as "three critical elements of the history of Southwest archaeology within which scholarly-public interaction has been particularly important: patronage, professionalism, and rationale" (p. xviii). Useful as these ideas are, they do not do as much lifting as Snead would like, and in the midst of a narrative filled with fascinating, quirky characters; and complicated, messy tales, they risk being reductive and a bit too neat.

That said, the book ably makes the case that the history of archaeology is central to shaping our understanding of the past. Snead is right to say that the "complex relationships between archaeology and society have often defied analysis" (p. xviii), and this book is a good corrective step in broadening that analysis.

STEVEN CONN
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MANSEL G. BLACKFORD. *Fragile Paradise: The Impact of Tourism on Maui, 1959–2000*. (Development of Western Resources.) Lawrence: University Press of Kansas. 2001. Pp. xiii, 277. \$35.00.

Maui is a microcosm of the tourist industry in the United States. The island's hotel and condominium rooms more than tripled between 1970 and 1980, as the disposable incomes of Americans and Japanese increased, and as jet airplanes made the Hawaiian

Islands accessible to the world. This book by Mansel G. Blackford analyzes how political, business, and environmental leaders shaped and reacted to Maui's transition from an economy based on sugar cane and pineapple to one based on travel and recreation. It also shows how competing groups of Maui residents debated the trade-offs between development and environmental protection.

The great virtues of the book are its skillful blend of political, economic, environmental, and cultural history; its thorough and extensive scholarship; its fair-mindedness; and its careful attention to Maui's most important business and environmental leaders, as well as to grass-roots political activists. The book makes a strong case for treating the Hawaiian Islands as part of the continental American West rather than as a tropical outpost of the United States. The focus is local; the implications and significance are regional, national, and even international. For example, Maui's recent history provides a case study of the relationship between "center" and "periphery" that will interest world systems scholars.

The book includes seven chapters that provide an overview of Maui's economy and an analysis of the major issues involved in the growth of tourism, including zoning and land use, water, electrical power, and the expansion of Kahului Airport (to accommodate direct flights from Asia and the United States). The book concludes with a birdseye view of communities in South Maui and Upcountry Maui. (South Maui is one of the fastest-growing tourist areas in the Hawaiian Islands, while Upcountry Maui remains largely agricultural.) Blackford introduces readers to Maui's major economic and environmental organizations, including the Maui Visitor Board, Maui Hotel Association, Maui Office of Economic Development, Maui County Council, Maui County Farm Bureau, and Maui Tomorrow (the island's largest environmental group). The Sierra Club, Audubon Society, and other national environmental groups played little part in the debate over tourism on Maui. The island's population was small compared to a large city in the continental United States, and its leaders knew each other so well that local groups dominated the debate over growth.

Blackford builds on recent studies of tourism in the American West, notably Hal K. Rothman's *Devil's Bargains: Tourism in the Twentieth-Century American West* (1998). When tourism first came to Maui on a large scale in the 1970s, it seemed to answer all of the island's economic problems, including its isolation and heavy dependence on one-crop agriculture. But, as elsewhere in the West, tourism in Maui strains existing transportation, water, sewage, and electrical systems; produces congestion that threatens the very amenities tourism seeks to exploit; and places the island's economic destiny far beyond the control of most of its residents. Tourism produces a permanent "service underclass," promotes the development of a nonresident class with little interest in local issues, contributes to soaring property values, and generally funnels

wealth to investors far removed from the beaches and resorts. Once tourism promised to free the American West from the boom-and-bust extractive economy of the nineteenth century, which in some places relied on wheat, in others cattle or sheep, and in others gold, copper, or uranium. But tourism on Maui has proved to be highly susceptible to economic recessions in California and Asia during the 1980s and 1990s. It brought money to Maui, but many island residents have argued that a diversified economy would provide a brighter future for local residents and the environment than attracting more visitors. Critics of tourism include promoters of hi-tech industries as well as those who want to expand specialized agriculture, including the growth of vegetables and fruits for local markets.

The future of tourism in Maui remains uncertain. Residents want stable, well-paying jobs that will allow their children to remain on the island. Unfortunately, adjusted for the cost of living, real wages on Maui are lower than on the mainland. In effect, tourism has substituted one "plantation economy" for another. The frustration of many island residents can be seen in a recent bumper sticker that reads: "If it's tourist season, why can't we shoot them?" This excellent book provides no ready answers to the problems generated by a tourist economy, but it does a superb job of tracing the evolution of that economy over the last four decades.

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JOHN F. KASSON. *Houdini, Tarzan, and the Perfect Man: The White Male Body and the Challenge of Modernity in America*. New York: Hill and Wang, 2001. Pp. viii, 256. \$26.00.

If middle-class, white American males of the late nineteenth and early twentieth centuries felt beleaguered, emasculated, and weakened by the social and economic transformations of their society, they had some accessible icons to provide them with means to overcome their plight. John F. Kasson identifies and imaginatively analyzes three popular figures: Eugen Sandow, Harry Houdini, and Tarzan, who together symbolized the strength, freedom, and superiority that helped create necessary new images of masculinity for the modern era.

Sandow, born in Prussia as Friedrich Wilhelm Muller, made the male body, and hence manhood, into performance art. From the early 1890s until his death in 1925, Sandow toured the United States and Great Britain displaying his nearly nude, muscular body and performing feats of strength in vaudeville and other entertainment venues. He also proselytized, insisting that his strength—and any man's strength—had to be achieved through strenuous yet scientific determination. Sandow demonstrated that a combination of body and will produced what Kasson calls a metamorphosis in a man that would reaffirm his masculinity in a time when men were losing respect. Sandow, says Kasson,

not only inspired men to emulate his body but also aroused them almost erotically to possess it.

Houdini, whom Kasson labels as the greatest escape artist in the history of illusion, was born Erik Weisz, the son of a Budapest rabbi. His performances, which extended from the 1890s to the 1920s, also involved metamorphosis, namely from helplessness to mastery, bondage to freedom, and in the process exerted a form of masculine prowess that complemented that of Sandow's strongman. Like Sandow, Houdini was self-made, overcoming his humble origins and learning better than anyone else how to defeat in his illusions and escapes what appeared to be the most dire threats to the human body. Also like Sandow, Houdini displayed himself nearly naked, offering his unclothed body to be chained, boxed, and otherwise impossibly restrained; but his nakedness created the image of underdog and vulnerability that made his escapes a reaffirmation of manliness. By constantly challenging and defeating death, Kasson asserts, Houdini symbolized masculine resistance to intimidation and domination, thereby expanding the male body's special powers.

Tarzan, of course, was a fictional character, the creation of Edgar Rice Burroughs. Restless and frustrated by business failures in Chicago, Burroughs turned to writing for pulp magazines and began writing a novel called *Tarzan of the Apes* in 1911, the first in a long series of stories about one of the most recognized characters of American popular culture. The offspring of British nobility who was orphaned when his parents died in the African jungle, Tarzan was raised by a family of apes. The character of Tarzan and his adventures in battling savages and beasts gave Burroughs the opportunity, in his words, to play with "the idea of a contest between heredity and environment" and in Kasson's words, "to test the nature of white Anglo-Saxon masculinity." Like Sandow and Houdini, Tarzan was a self-taught, self-made man. His conquests and unbridled individualism made him "lord of the jungle," who gave male readers symbols of the independence and victory they longed for in the modern technological age.

Throughout the book, Kasson shows his capacity for uncovering meaning in what on the surface may appear to be mundane acts and symbols. He makes his analyses of aspects such as Sandow's body, Houdini's impossible confinements, Tarzan's jungle feats, and all three characters' nakedness and whiteness all the more convincing by a copious use of photographs and illustrations. At times, however, Kasson stretches his interpretation a bit too much. For example, he supposes that artistically posed photographs of Sandow's nearly nude body "in all likelihood" were more appealing to men than to women (p. 67). Kasson also speculates that when Houdini's father took him to his first magic show, in which the magician seemingly sliced up a man's body, the youth drew a subconscious connection between the illusion of mutilation and his father's rabbinical role in ritual circumcision (p. 85). He iden-

tifies significant cultural symbols in the Tarzan stories when Burroughs himself admitted that he intended his work merely as an imaginative romantic adventure. Moreover, Kasson only cursorily refers to the middle-class orientation of his focus. It is likely, for example, that working-class men appreciated John L. Sullivan more than they did Sandow. These minor criticisms, however, pale in the context of this engaging and influential book.

HOWARD P. CHUDACOFF
Brown University

SHEILA TULLY BOYLE and ANDREW BUNIE. *Paul Robeson: The Years of Promise and Achievement*. Amherst: University of Massachusetts Press. 2001. Pp. 521. \$39.95.

To write a biography of an African American in the early twentieth century is to confront two analytical problems. How does one fairly assess achievements of the subject in light of the racial barriers in his or her path? And how does one account for success in such an unpromising context? Sheila Tully Boyle and Andrew Bunie dodge neither of these problems as they present the first four decades of the life and career of Paul Robeson.

The study covers the years between 1898 and 1939, during which Robeson emerged as a college football sensation, earned a law degree, and gained world renown as a singer and actor. The authors answer the first critical question by situating Robeson in turn-of-the-twentieth-century New Jersey, where his parents, William, a former slave, and Maria Bustill, a daughter of one of black Philadelphia's oldest and most respected families, struggled against both racial segregation and black elite prejudice against dark-skinned freedmen. Much more than racial prejudice troubled young Paul and his siblings. The elite Bustills ignored them; William rose to a respected position in the church he pastored only to be felled by scandal; and Maria died tragically under circumstances brought on by the family's poverty and her declining health. As the youngest child, Paul forged a bond with his father that inspired him to excel in all endeavors. The authors' research into the New Jersey communities where the Robesons lived and their attention to father-son dynamics and class/color prejudice make Paul's story much more compelling and human than a struggle against racism. Without diminishing the obstacles before him, they present Robeson as a person shaped and driven by love, intellectual curiosity, and will.

The authors avoid the strategy of cataloguing their subject's achievements while chalking up any flaws to the unfavorable racial climate. Robeson's exploits on the college gridiron, in the classroom, behind the podium, and as a performer occurred against a backdrop of youthful sloth, womanizing, and an insatiable desire for adoration. Race prejudice, of course, abounded. Some football teams refused to compete against Robeson; American dramatists imagined few

roles for black actors; all-spiritual concerts eventually drew sneers from critics calling for proof of Robeson's mastery of "serious" music; and white women, happy to "slum" with a famous black man, would not dream of marrying him. But what marked Robeson's young adulthood was not hardship so much as indecision and drift. His physical prowess, keen intellect, rich voice, and jovial personality made everything seem possible and nothing the particular focus of his ambition.

Robeson's decision in 1921 to marry Essie ("Essie") Cardozo Goode, an ambitious, light-skinned, upper-class woman, set him on a course toward stardom. He abandoned law (after finally earning a degree from Columbia University in 1923) and professional sports to pursue a career as a performer, and Essie managed him. Praise for his performances in Eugene O'Neill's *The Emperor Jones* and *All God's Chillun* opened the door to other parts. But accolades from white critics were matched by dismay from black ones. Robeson's forceful presence and voice could not mask the African-American stereotypes, and black critics wondered when good parts would come and why Robeson took demeaning roles. This kind of commentary, along with Essie's sensitivity to racial slights and his accompanist Larry Brown's refusal to perform in the South, prompted Robeson to leave the United States. In England he believed he had found a free and egalitarian society that would judge him by his merits, not by the color of his skin. He gave concerts at prestigious venues and made pro-imperial adventure films, believing that his strong black characters would be positive role models for black audiences. Critics let him know otherwise. Trips to the Soviet Union renewed his hope for an egalitarian society, but they did not expose the gap between his art and his struggle for racial justice.

The Spanish Civil War marked a turning point in Robeson's life. As sudden and powerful as a religious conversion, Robeson embraced the republican cause and transformed his professional life. The relationship between personal success and racial uplift—always vaguely defined—became problematic. Personal success could not dismantle power structures that trapped people of color, and no great individual achievement could make democratic nations support democratic movements overseas. The reward for political commitment, Robeson realized, would be to strive for the egalitarian world he wanted to inhabit.

The book ends abruptly in 1939 with Robeson at the peak of his popularity and energized by the correspondence between his career and his politics. The authors only gesture at his undoing in the years after 1940. Given the meticulous research that went into this excellent biography, one can only hope that Boyle and Bunie will devote another volume to the years of Robeson's demise.

SUSAN CURTIS
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NANCY GENTILE FORD. *Americans All! Foreign-born Soldiers in World War I*. (Texas A&M University Military History Series, number 73.) College Station: Texas A&M University Press. 2001. Pp. xiii, 194. \$32.95.

In this book, Nancy Gentile Ford offers a much needed corrective to the traditionally held view that the American army mimicked civilian society by adopting a harsh, one-hundred-percent Americanism approach toward training foreign-born soldiers during World War I. Nearly half a million foreign-born soldiers of forty-six different nationalities served in the wartime army. With one in five soldiers foreign-born, including many who did not speak English, the army faced a serious challenge training these men for battle. In marked contrast to the conformity demanded of foreign-born civilians on the homefront, Ford's meticulous research in official army records reveals that army officials demonstrated a remarkable sensitivity to and respect for Old World cultures when formulating the training and recreational regimes of foreign-born troops. With the help of Progressive settlement house workers, reform-minded army officers, and civilian ethnic leaders, the wartime army strove to meet the distinctive cultural demands of immigrant troops. Reformers and ethnic leaders argued convincingly that respecting the cultural traditions of the foreign-born would make them more effective combatants, an argument that helped persuade the War Department to institute a host of reforms intended to keep morale high among foreign-born troops. Reforms included meeting religious needs, such as providing Polish troops with Polish-speaking priests to say Mass and giving Jewish soldiers unleavened bread during Passover. The War Department translated propaganda pamphlets into a multitude of languages and sent bilingual officers to give morale-boosting speeches that highlighted the role of different ethnic groups in the American war effort. The importance of interpreting films with English subtitles became clear when groups of immigrant soldiers shown the film, *Keeping Fit to Fight*, misconstrued its anti-venereal disease message and instead concluded that the army was showing them smutty films. In the last few months of the war, the army began temporarily segregating the foreign-born into ethnic-specific development battalions. In these units, foreign-born soldiers received military instruction under the guidance of bilingual officers and studied English before going on to serve as replacement troops in France.

Ethnic leaders, Ford argues, had their own reasons for helping the army train the foreign-born. Many were interested in defeating Germany and freeing their homelands from the control of the Austro-Hungarian Empire. Through their wartime activities, civilian ethnic leaders helped foreign-born soldiers develop a dual identity that allowed them to pledge allegiance to the United States without giving up valued cultural customs and native languages.

As Ford notes, military officers and ethnic leaders left a paper trail detailing their reform efforts. Speaking a multitude of languages and often illiterate, it is more difficult for rank-and-file immigrant soldiers to tell their own stories. This book, therefore, primarily recounts the experiences of foreign-born troops through the accounts of those officials striving to improve the treatment they received in the training camps. Ford relies heavily on the records of the Foreign-Speaking Soldier subsection (FSS) of the Military Intelligence Division. Founded initially to investigate the loyalty of immigrant soldiers, the FSS came under the influence of reformers more interested in morale-building than counter-espionage. A bit more detail about the investigations of supposedly disloyal foreign-born troops, however, would help to clarify whether reforms replaced, or were carried out alongside, undercover investigations indicative more of suspicion than trust.

Ultimately, the army's moderate approach had little effect on civil society. The War Department's publicity promoting positive images of loyal and dedicated foreign-born troops competed poorly with the host of negative ethnic stereotypes that filled the wartime press. Unswayed by the illustrious combat record of the foreign-born, civilian nativists pushed through strict immigration restrictions in the 1920s.

Ford successfully challenges longstanding assumptions about the immigrant experience during World War I and documents a key aspect of the army's modernization during the war. She also provides a much needed examination of the difficulties encountered in drafting the foreign-born. This is an important book whose blending of military and social history will interest historians of the military, immigration, and Progressivism.

JENNIFER D. KEENE
University of Redlands

ELMER R. RUSCO. *A Fateful Time: The Background and Legislative History of the Indian Reorganization Act*. (Wilbur S. Shepperson Series in History and Humanities.) Reno: University of Nevada Press. 2000. Pp. xv, 363. \$44.95.

Over time it has become increasingly clear that the passage of the Indian Reorganization Act (IRA) in 1934 as a part of President Franklin D. Roosevelt's Indian New Deal was indeed a fateful act. All agree that it stands as one of a handful of the most important pieces of legislation affecting Indians, that it reversed long-standing policies of forced assimilation, and that it tried to base policy on the new ideology of cultural pluralism. But historians remain divided on the question of whether or not the IRA succeeded in its goal of greater Indian self-determination. Most scholars before the mid 1970s emphasized its positive achievements. Since the late 1970s, however, more and more have found faults and failure.

Elmer R. Rusco's exhaustive study of the IRA

acknowledges its shortcomings, but overall he sides with supporters. In the preface, Rusco provides an excellent overview of existing literature on the IRA. In fact, much of this book covers the same ground as works by Kenneth R. Philp, Lawrence C. Kelly, Graham D. Taylor, Vine Deloria, Jr., and Clifford M. Lytle. Nonetheless, Rusco succeeds in finding important new areas of inquiry, insight, and interpretation, and he also enriches our understanding of dimensions of the IRA already studied by others.

Rusco argues that, by the late 1920s, policy makers had come to a consensus that the forced assimilation policy of the past had failed. But no agreement existed on what alternative should replace it. Some scholars have interpreted the Indian policies of the Herbert Hoover administration under Commissioner of Indian Affairs (CIA) Charles Rhoades as transitional, prefiguring Roosevelt's Indian New Deal. Rusco instead sees the Hoover administration's policies as an attempt to implement an ideology entirely different than either forced assimilation or cultural pluralism. The famous Meriam Report, published in 1928 after a careful study of reservations, revealed deplorable conditions in vivid detail, and it called for the government to spend much more money on Indians. Hoover appointees to the Bureau of Indian Affairs (BIA) believed that once the government followed through on Meriam Report recommendations, Indians would assimilate into American life largely on their own.

Other IRA scholars have concluded that the IRA succeeded primarily because of the tireless efforts of Roosevelt's CIA, John Collier. Like others before him, Rusco emphasizes the tireless drive of the former Indian policy activist. But Congress had much less interest than Collier in resurrecting Indian self-government and tribal culture. Even after considerable compromise, the bill made it through Congress only because of backing by Secretary of the Interior Harold Ickes and because of President Roosevelt's political clout with a Democratic majority in the Congress.

Rusco also finds roots of the IRA in places others have little explored. He sees precedents in the court system, in ideas coming from Indian groups themselves, and in the policies and administrative practices of the government's dealing with Indians through the BIA. He argues that the judicial system had long acknowledged that tribes retained a limited sovereignty that Congress had long ignored. Over many years, the BIA had tacitly accepted some degree of separate sovereignty as well when it worked informally with tribal governments in implementing congressional Indian policy.

On the central question of whether the IRA represented just another assimilationist attempt to impose Euro-American institutions on Indians or a genuine acceptance of cultural pluralism and a move toward Indian self-determination, Rusco finds a confusing mixture of both. Breaking the paternalistic grip of the BIA and ending Indian dependency has been frustratingly slow. But Rusco credits the IRA for at least

providing tribes with the means to develop effective internal leadership. This enabled Indians to gain more control over their individual and community lives and to define more on their own terms the nature and pace of their cultural and political integration into the United States.

Scholars of Indian affairs will appreciate this book's contribution to our understanding of the evolution of Indian communities in the twentieth century and our understanding of U.S. government-Indian relations. But it sometimes overwhelms with details and examples, and its organizational style of summarizing conclusions at the end of each chapter and of dividing chapters into captioned subcategories sometimes results in redundancy and choppy transitions.

LARRY BURT

Southwest Missouri State University

DAVID E. BERNSTEIN. *Only One Place of Redress: African Americans, Labor Regulations, and the Courts from Reconstruction to the New Deal*. (Constitutional Conflicts.) Durham, N.C.: Duke University Press. 2001. Pp. xiii, 191. \$39.95.

Inspired by a 1998 lecture at the Cato Institute on "the effects of facially neutral economic regulations," by legal studies at Yale, and by a John M. Olin Foundation fellowship (p. xi), David E. Bernstein has produced a revisionist account of job discrimination against African Americans in the context of neoclassical economics, which holds that wages are flexible in a free labor market. The major culprits in this saga of discrimination, the author asserts, were those who used "political power at the expense of those without" it (p. 5), particularly unions allied with governments, which interfered with the market at the expense of black workers. The key legal case, *Lochner v. New York* (1905), by invalidating a New York law limiting bakery employees to a sixty-hour week, upheld the free market principle. It also labeled the New York law as class legislation, because unionized German workers benefited at the expense of unorganized Jewish and Italian immigrants. Thus "Lochnerism" is used throughout the study in reference to all "liberty-of-contract jurisprudence" (p. 2). According to Bernstein, African Americans, discriminated against and disfranchised, benefited from *Lochnerian* decisions. Nevertheless, *Lochner* subsequently faced withering criticism and was discredited by "statist Progressives and Legal Realists" (p. 2).

The author's list of laws and regulations inhibiting African Americans is legion. Emigrant agent acts, promoted by southern planter "cartels" (p. 9), sought to restrict intrastate mobility in order to maintain below-market wages. Prohibitive tax levies and vigorous enforcement, ultimately upheld by the courts, inhibited labor agents from enticing blacks into high wage states to the west, although a great migration took place despite the legal impediments. Licensing laws adversely affected African-American plumbers,

barbers, dentists, and physicians as well as Chinese laundry operators in California. Racist railroad unions, granted "monopoly power" (p. 46) by the Railway Labor Act (1926), drove blacks out of the industry, and the Norris-LaGuardia Act (1932) curtailed the use of labor injunctions and ended yellow-dog contracts. Both had "worked to the benefit of African American railroad workers" (p. 54). Prevailing wage laws imposed by the Davis-Bacon Act (1931) kept blacks from competing for federal construction jobs. Historians have examined these topics, sometimes extensively, and the adverse effects upon blacks are generally familiar. The author, in this synthesis, has fleshed out the material with particular attention to federal and state laws and recent scholarship in the law journals.

In an area as complex as race relations in the period under study, in which discrimination and racism permeate the entire society, government action or inaction can become easy targets for blame. The author and many other writers have made the case that, regarding job opportunities for African Americans, laws and regulations promoted by special interest groups made their situation worse. While harming minorities, however, the same government action often enhanced the bargaining power and wellbeing of the majority, although in promoting their goals white railroad and construction workers never attained the kind of "monopoly power" (p. 93) that the author alleges. Nor does recent research support the conclusion that minimum wage laws always create unemployment. Of course, the basic causes for the African-American plight remained endemic in the society as a whole: discrimination and racism. In their absence, blacks and other minority railroad and construction workers, for example, could have benefited along with the white majority. The author's almost single-minded focus on government as the cause unfortunately blurs such distinctions.

Nevertheless, in rendering this neoclassical interpretation of relevant economic events, the author presents arguments on both sides, although little new content is in evidence.

MERL E. REED

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TIMOTHY J. MINCHIN. *The Color of Work: The Struggle for Civil Rights in the Southern Paper Industry, 1945-1980*. Chapel Hill: University of North Carolina Press. 2001. Pp. x, 277. Cloth \$55.00, paper \$24.95.

In this fine study of paper industry workers, Timothy J. Minchin attempts to show how both employers and white union leaders tried to maintain a segregated workforce, even after the passage of the Civil Rights Act of 1964, and what strategies black workers adopted to fight unequal treatment. Strangely, Minchin prefaces his study by using Alan Draper's statement that the question of whether "black trade unionists saw

their unions as a 'legitimate or relevant conduit for their civil rights demands' has yet to be explored by historians." Surely the works of Eric Arnesen, Rick Halpern, Daniel Letwin, and Brian Kelly have attempted to address such a question, although as Minchin explains, little attention has been paid to paper workers generally. Approximately 150,000 workers labored in the southern paper industry in 1960, fourteen percent of whom were black (p. 13).

Minchin highlights how both the industry and its attendant unions were racially segregated. African Americans were barred from most production jobs and consigned to the poorest paying and physically demanding jobs. As one black worker stated, "you either pulled something or pushed something" (p. 36). Although black workers enjoyed union representation and recognition, they were placed in Jim Crow locals with little voice over their work conditions and wages. The experience of black workers was not affected by the size of paper plants; the result was nearly always the same: large differentials in pay.

Minchin identifies decades of inequality and frustration experienced by black paper workers. The bulk of the study shows how black workers used the Title VII of the 1964 Civil Rights Act to counter years of discrimination. Specifically, black workers brought class action suits to the Equal Employment Opportunity Commission (EEOC). It is these suits, in tandem with the oral histories, that provide the basis of Minchin's treatment. Other federal agencies appealed to included the Office of Federal Contract Compliance (OFCC) and the Department of Labor. Many of the early cases revolved around different seniority lists, which had trapped African-Americans in menial jobs due. Using the EEOC, blacks effectively challenged such lists and other barriers to better-paid white jobs.

Company officials argued that many of the white jobs required educational skills that black workers did not have. As in other industries, companies created new barriers to black promotion. Tests were instituted to forestall black entry into skilled production jobs. Additionally, white workers refused to train those few fortunate black workers who had managed to get promoted. So a second period of agitation followed where tests and worker attitudes were challenged. Leading the charge were black leaders from the Jim Crow union locals. Once the suits were begun, white union attitudes shifted. White union officials tried to adhere to new set of relations, not least because they were found financially liable for past discrimination. Thus in an attempt to avoid financial penalties, they became more forceful in fighting discrimination. Both employers and union officials reluctantly acquiesced to the dismantling of racism within the paper plants. To avoid outright conflict with white workers, they frequently argued that their hands were tied by federal law. Slowly other major barriers came down too, like segregated seating in factory canteens as well as divided bathrooms and washrooms. Battles continued

into the 1990s over the issue of promotion and white employer attempts to maintain a racial pecking order.

The study is a clear portrayal of institutionalized segregation in the paper industry and African-American attempts to dismantle its ugly and restrictive manner. I have a few questions, however. While explaining well the tactics used to eradicate discrimination, Minchin ignores the agencies that rule on and enforce racist strictures. The reader is left with no clear understanding of who in the EEOC, OFCC, and the Department of Labor took on the mission of desegregating the paper industry. The book also at times reads more like a series of case studies. The narrative flow tends to move back and forth, taking on a thematic approach that tends to leave the reader adrift. These criticisms aside, the study contains a welter of information and testimonies of how segregation was enforced in the paper mills, and how black workers and their local leaders took on such barriers and eventually triumphed. The book is sure to join other standard texts on how African-American workers challenged their second-class status in the workplace.

COLIN J. DAVIS

University of Alabama at Birmingham

CLETE DANIEL. *Culture of Misfortune: An Interpretive History of Textile Unionism in the United States*. Ithaca: Cornell University Press. 2001. Pp. x, 327. \$49.95.

Was industrial unionism the panacea that the Congress of Industrial Organizations (CIO) and those historians who praise it claim? Maybe not, suggests Clete Daniel. Textile unionism, he argues, has been a "culture of misfortune" earmarked by substandard wages, miserable working conditions, regional animosity, ineffective labor organizations, and ruthless corporate might. His "interpretive history" is short on glory and long on failure.

Daniel persuasively argues that textile unionism was moribund from its inception. Textile manufacturers pioneered in employing a deskilled labor force of marginalized workers. This made unity elusive, with nineteenth-century workers joining unions as much out of "bandwagon" mentality as solidarity (p. 16). By the twentieth century, the culture of misfortune was solidly in place, frustrating efforts of unionists, regardless of their political ideology or organizing principles. The largest union, the United Textile Workers (UTW) held little sway in the craft-dominated American Federation of Labor (AFL). Daniel sees famed strikes like those in Lawrence (1912), New Bedford (1928), and Gastonia (1929) as high drama but organizational nightmares that the UTW mostly made worse through ineptitude, internal squabbling, and a lack of resources.

The rise of industrial unionism, the New Deal, and World War II offered unparalleled opportunities for many, but not textile workers. Daniel adds fuel for revisionist historians who argue that the CIO was

essentially a paper tiger. The UTW became the Textile Workers Organizing Committee-CIO (TWOC) but, Daniel claims, the TWOC had little understanding of textile "organizational structure or culture" (p. 67). New England workers complained that the CIO efforts reduced northern wages rather than raising southern rates. Although "Southern millhands were as eager as their northern counterparts to hear the CIO's message" (p. 79), the federation misjudged the depths of southern anti-unionism. Much of the TWOC's southern strength consisted only of signed pledge cards, and even modest gains were wiped out by the recession of 1938. By 1939, the newly christened Textile Workers Union of America (TWUA) looked like the old UTW. World War II capital/labor arrangements helped the TWUA win contracts it could not have achieved alone, though most southern firms refused to buckle. As Daniel notes, "the TWUA was . . . even more emphatically a northern-based union when the war ended" (p. 144).

Daniel could have stopped with the armistice, as his remaining pages chronicle a downward spiral previously examined by historians like Steve Dunwell, Timothy J. Minchin, and Barbara S. Griffith. Echoing Griffith, Daniel declares that the CIO's "Operation Dixie" was effectively dead by late 1947, but he could have done more to link this to the collapse of northern textile manufacturing. By the 1950s, aging northern operations fled south, where red-baiting, the Ku Klux Klan, right-to-work laws, southern preaching, and an individualist ethos proved more powerful than industrial unionism.

Daniel curiously devotes most of two chapters to struggles between TWUA president Emil Rieve and southern organizer George Baldanzi. His implication that the TWUA should have cast its fate with the latter is problematic given staggering losses in the Piedmont the year before the union ousted Baldanzi. Lost is serious consideration of Rieve's charge that the South was unorganizable. Also given short shrift is the CIO's war on militancy, its dependence on the Democratic Party, and its assent to Cold War policies. Daniel correctly criticizes the TWUA's "poor leadership" (p. 245), but it is doubtful that stronger leaders could have bucked prevailing trends within the CIO or American society. By 1955, "bargaining from weakness" (p. 246) was no longer a strictly southern phenomenon. Daniel does, however, astutely suggest that textiles were a harbinger of the fate of other industrial unions.

Celebrated postwar victories like those over J. P. Stevens in 1980 and Fieldcrest Cannon in 1999 have excited many, but Daniel asserts that only a "Pollyanna" would equate recent victories with securing "textile unionism's future." He politely ends his book with the comment that over 160 years of frustration "might forever challenge the faith of those who endured textile unionism's cause, but never enough . . . to extinguish it forever" (p. 281).

Daniel's conclusion is more considerate than considered. The Fieldcrest victory, for example, was that

of a truncated union (UNITE) against a downsized corporation also formed via merger. Southern workers now face the same capital flight that wrecked the northern textile industry; the region shed a third of its textile jobs in the 1990s and the total number of Piedmont workers is fewer than the TWUA organized in the 1940s. Daniel probably got it right in the book's early pages. Once the culture of misfortune became entrenched, all that was left to fight over was a pile of loose threads.

ROBERT E. WEIR
Bay Path College

ROGER M. OLIE and DIANA DAVIDS OLIE. *Oil and Ideology: The Cultural Creation of the American Petroleum Industry*. (The Luther Hartwell Hodges Series on Business, Society, and the State.) Chapel Hill: University of North Carolina Press. 2000. Pp. xviii, 305. Cloth \$49.95, paper \$18.95.

Half a century ago, in his Pulitzer Prize-winning *The Age of Jackson* (1945), Arthur M. Schlesinger, Jr., crystalized the Progressive view of business when he wrote that "liberalism in America has been ordinarily the movement on the part of the other sections of society to restrain the power of the business community" (p. 505). This volume on the oil industry, which ends in 1945, argues that such negative ideological views have resulted in a counterproductive relationship between the industry and government. Roger M. Olien and Diana Davids Olien present a history of socially constructed negative attacks on the petroleum business. Revision is needed, they argue, because the alleged sins of John D. Rockefeller's Standard Oil Company have not only shaped the popular image of this industry ever since but led to the evolution of a poorly designed and failed public energy policy based on moralistic and false impressions.

The authors tell us that this study is "neither a history of the petroleum industry nor an exhaustive history of policy making but a history of the development of discourse affecting both" (p. xi). They utilize the fashionable concepts of social construction and gendered language analysis, but the book is really about more basic stuff—an overview of perceptions and public policy affecting the oil industry from the antitrust attacks of the late nineteenth century through the more complex terrain of the New Deal and World War II. It is interesting to learn how the public pilloried Rockefeller by word and cartoon and how critics applied feminine language to challenge his manhood. But in critiquing these attacks on one level, the authors have perhaps substituted one set of simplistic notions for another. Not only did Ida M. Tarbell, somewhat grudgingly perhaps, also demonstrate admiration for the business acumen and achievement of the Old Standard Oil, so have legions of authors and business historians over the years. It has been a very long time since a one-dimensional negative view of the oil industry has dominated the literature.

There are also surprisingly limited discussions of the economics of the antitrust debate, Theodore Roosevelt's views on the subject, and the differing approaches to monopoly that evolved from the New Freedom through the New Deal. There is a very rich and valuable treatment, however, of the conservation issue in the 1920s and 1930s and how perceptions of physical and economic waste penetrated policy debates.

The central question is whether "dysfunctional" policies that have evolved to challenge and regulate the petroleum industry have been inadequate or simply wrong because "channels of discourse" about the industry have been based on moralistic value judgments. Although their detailed analysis ends in 1945, the authors speculate briefly in their concluding chapter that the problem has continued into the more recent past as evidenced, for example, in criticism engendered during the "energy crisis" of the 1970s. The Standard Oil giant did come to symbolize the liberal critique of monopoly and "big business" for generations of Americans and helped to shape popular cultural views of the wider industry. But were all of these perceptions flawed? Rockefeller should be studied as a creative and pioneering businessman who imposed order on a chaotic economic environment, but he and his associates were also frequently quite ruthless enemies of free competition. The Oliens correctly point out the overzealousness of the classic critiques of Standard Oil by Henry Demarest Lloyd and Tarbell, for example, but do the Rockefeller near-monopoly and the current oligopoly of multinational oil firms really need an academic defense at this point in the twenty-first century? Given the Oliens' earlier work on independent Texas oilmen, it is also surprising that one gets a rather monolithic view of the oil industry in this volume. Which oil industry are we talking about? The goals and politics of the independent oilman are often quite different from those of the vertically integrated dominant firms, and such differences, while addressed, are often blurred in this study.

The Oliens are dismissive of Gerald D. Nash's classic study, *United States Oil Policy, 1890-1964* (1968), but I would argue that it still remains a better guide to understanding the currents of both political debate and intra-industry struggles over policy. Moreover, the large body of research on the associative state published since 1968 has bolstered, not weakened, Nash's earlier view that business-government cooperation has been a more powerful and lasting force in the oil policy arena than has adversarial attack.

AUGUST W. GIEBELHAUS

Georgia Institute of Technology

SCOTT HAMILTON DEWEY. *Don't Breathe the Air: Air Pollution and U.S. Environmental Politics, 1945-1970*. (Environmental History Series, number 16.) College Station: Texas A&M University Press. 2000. Pp. 321. \$39.95.

Despite how air pollution wafted onto the central stage of politics in the post-World War II United States, historians of this era have largely ignored it. Scott Hamilton Dewey's book shows how much we have missed. Dewey's work places him in the front ranks of what has recently become a burgeoning field of inquiry; it is the first full-fledged historical treatment of dealings with aerial contaminants from the 1940s through the 1960s. He writes of how today's regime of atmospheric regulation first took shape, as "smog" got in our eyes and onto the radar screen of media and local government officials, first in California but also later in other locales. Besmirched breezes soon wound up under federal control with the passage of the Clean Air Act; this book offers the most compelling explanation yet of why.

Three separately handled case studies make up Dewey's story: cutting-edge Los Angeles, lagging New York City, and rural central Florida. Los Angeles bore first witness to the new type of pollution brought on by massive use of automobiles: the brown stinging mist that became known as "smog." New York, typical of the nation as a whole, grappled fitfully through the 1950s and early 1960s with what its officials viewed through the older paradigm of "smoke control": visible particles that resulted more from coal than from gasoline burning. Only as they began to have their own smog studies and alerts during the mid-1960s did a widespread sense of urgency arise, most notably in the 1965 mayoral election. If anything, the airborne wastes from central Florida's phosphate industry roused earlier worries, for they included fluorine, which destroyed the neighboring trees and grass on which citrus growers and cattle raisers depended. Public concern surged from the mid-1950s onward and brought federal studies and interventions.

Telling these three stories one at a time, covering the same chronological ground thrice, Dewey trades analytical integration for local detail and texture. He effectively shows how widely pollution problems came to be shared but offers fewer reflections on the geographic differences either in contaminants or awareness of them. His three stories connect mainly through how local agencies dealt with the pollution problem, whether by "educationalist" or "prosecutionist" approaches to regulation. By his estimation, "educationalists," like New York City's Leo Greenberg in the 1950s, usually wound up accomplishing less than "prosecutionists" in the Los Angeles Pollution Control District, though the "more" achieved by the latter verges on circularity: more prosecutions. Dewey does explore the distinctive features of the pollution problem in each case, and the adaptations in scientific understanding and technical practice that were required to contend with them. Yet the main vein of historical reflection here is a kind of running argument against those who dominated these agencies. They "dismissed the observations of laymen too hastily" (p. 212); contrary to what they assumed, says Dewey, air pollution control "is primarily a matter of public

policy, politics, and law, not merely a science or engineering project" (p. 51).

For this reviewer, Dewey's analytic grip on these stories seems too easy, brushing past important complexities. Condemning those regulators who would emphasize "education," he offers little explanation as to why they remained so resistant to the "lessons" they should have learned. The approach also involves back-handed slaps at the public and lay actors he is otherwise inclined to champion. Praising their prosecutionism as the engine of regulatory progress, he slights their observational prowess. There is little recognition here of how the drawbacks of contemporary expertise provided grounds for lay confidence: how, for instance, with a novel problem like smog, the vantage points of experts and lay people could seem equally uncertain, their conclusions equally valid. Although he asserts the primacy of lay pressures on agency effectiveness, Dewey nevertheless only broaches antipollution activism at or near the end of his case histories, as reactive "folklore."

While this framework enables an adept telling of each regional story, a fuller assessment of their significance will require more robust terms of regional comparison and a richer sense of historical context. Dewey mentions the Progressive-era smoke control movement as a predecessor to postwar events but does not show how its lineage was reflected in postwar grappling with pollution problems, old as well as new. We learn little of how postwar agencies, officials, and advocacy groups compared to earlier ones or to other regulation of environmental health hazards. Nor is there much effort to situate pollution control within the more general history of government-business relations. Dewey does offer important correctives to Samuel Hays's narrative of postwar environmentalism. Among a lay public outside the Sierra Club, he asserts that pollution politics took precedence over wilderness issues early on, and that these popular worries about pollution had every bit as much to do with "health" as with "aesthetics" and were tied to the rising "ecological consciousness" embodied in Rachel Carson's *Silent Spring* (1962). Given this argument, however, Dewey's neglect of the historiography of public health and medicine stands out as a particular weakness. This literature might have helped him explain a contrast that he treats as virtually unproblematic: between those health scientists and officials who guided the pollution control agencies and those other medical professionals, classified here as "folklorish" activists, who pressed for more "prosecutionist" measures.

Despite these theoretical and contextual deficits, however, Dewey's narrative transports our understanding of postwar pollution politics to a new level. The stories he has dug up show how widespread the local concerns about soiled air were, and how intractable pollution proved to local solution. Raising questions about just what postwar environmentalism was, this book points the way toward a new appreciation of how it culminated in one of the most sweeping exten-

sions of Washington's economic grip. Overall, his is a pregnant story, one whose further exploration and interpretation should provide for important revisions in our understanding of what postwar American politics was all about.

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LAURA MCENANEY. *Civil Defense Begins at Home: Militarization Meets Everyday Life in the Fifties*. (Politics and Society in Twentieth-Century America.) Princeton: Princeton University Press. 2000. Pp. x, 213. \$29.95.

It is just as well the Cold War brought no nuclear exchange for, as Laura McEnaney shows, in the realm of civil defense, the needs, the means available, and national values combined awkwardly to produce so "incoherent" a program that the results would have been calamitous. Her book probes the efforts of the Federal Civil Defense Administration (FCDA) to "domesticate" nuclear-age defense, framing a policy that deeded to individual householders the chief responsibility for protection against the Bomb. Such a devolution, she suggests, was the only way there would be any civil defense (CD) program at all.

The book focuses on the political culture in which CD activists sought to devise a program, pry support from a stingy Congress, and evangelize the public. Its emphasis is on marketing strategies and also on the dual cooptive process by which some sectors of society not only were mobilized by the FCDA but also exerted influence over its programs themselves. As a result, some interest groups were simultaneously influenced by and, on their part, shaped and vernacularized programmatic elements to their own ends.

It has long been noted that the nuclear menace elicited a puny program for civilian survival, one short on physical and long on symbolic and ideational structures. To explain in part this parsimonious response to so daunting a threat, McEnaney argues that political leaders were dead set against any new "welfare" program. Some post-attack welfare was inevitable: sheltering, feeding, and provision for refugees and workers transplanted to keep U. S. industry humming. The poobahs of civil defense sought—and, given scanty appropriations had little choice but—to avoid designing a program that conveyed any sense of citizen entitlement. A consensus that civil defense should not fall prey to "military rule" also had a limiting effect. Perforce, the program had to rest heavily on the orthodoxy of self-help and individualism.

An approach anchored in the "traditional" single-family home was politically necessary and, happily, congruent with the reigning ideology of the self-sufficient nuclear family with working father, full-time homemaker mother, and kids. But this had shortcomings. Many mothers worked away from home. Moreover, policy called for children to be evacuated to-

gether with their schoolmates, dads to skedaddle directly from work, and mothers to flee separately, so families would be at least temporarily ruptured. The problems inherent in an evacuation strategy prompted a shift in the mid-1950s toward the one-family home shelter. The alternative, mass communal shelters, worried civil defense planners: different races and classes mingling in mass shelters seemed to promise trouble. FCDA policy makers were alert to the hazards of planning for a "polyglot population." They knew that emphasis on individual home shelters risked being seen as inequitable, for not everyone lived in, let alone owned, a single-family home—or had a basement.

Despite the modest resources civil defense controlled, various constituencies sought inclusion in its councils. Women's organizations, especially clubs, sought influence; since a home-based defense relied heavily on women, they could not be denied, and they became active in civil defense planning. Organized labor agitated to make sure that collective bargaining survived in any postnuclear future. African Americans wondered how well a segregated society would serve their civil defense interests. Truman's choice of Millard Caldwell, former segregationist governor of Florida, to head the FCDA did not reassure them. Although their opposition to the nomination proved unavailing, they, too, won modest leverage in the program. The role of women was more prominent than that of labor or black civil rights groups, and those sections of the book dealing with women are thus more revealing—no surprise given the strong emphasis on the home as the first line of nuclear defense. Influence over civil defense came at a price, the author argues: acceptance of Cold War anticommunism and some complicity in the militarization of American life.

The thesis in this book is a heavily argued one. Sometimes the dialectic seems to outrun the data. It is not always clear how much civil defense actually happened. There were speeches, research, prognostications, and drills, but, as McEnaney is fully aware, it was not easy to determine how many were heeding the message. (One survey in 1960 documented the existence of only 1,565 home shelters.) Since the history depicted often transpired on a symbolic, even ceremonial level, it would have been profitable if the book had examined Alert America, an FCDA-sponsored program by which truck convoys brought nuclear-age educational exhibits to the public and sought to sign up civil defense volunteers. The Ground Observer Corps also merits attention. Nonetheless, the argument is admirably well researched, usually convincing, often imaginative, and always interesting.

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KENNETH D. ROSE. *One Nation Underground: The Fallout Shelter in American Culture*. (American History and Culture.) New York: New York University Press. 2001. Pp. x, 313. \$28.95.

In Stanley Kubrick's classic film *Dr. Strangelove, or How I Learned to Stop Worrying and Love the Bomb* (1964), the occupants of the War Room salivate at the prospect of a postapocalyptic world where deep mine shafts become sexual playgrounds. They are eager to help repopulate the planet—even if it means prodigious sexual labor and abandoning monogamy. This book demonstrates that most Americans were considerably less enthused at the thought of life in fallout shelters. With clear prose, wonderful illustrations, and wide-ranging evidence, Kenneth D. Rose examines the intense national debate about the morality and efficacy of fallout shelters during the early Cold War.

These heated discussions were sparked by John F. Kennedy's response to the Berlin crisis in mid-1961. Determined to protect West Berlin and to prove his mettle against Soviet Premier Nikita Khrushchev, Kennedy called for a \$3.24 billion increase in defense spending and a \$207 million civil defense initiative that would identify public and private spaces that could serve as fallout shelters in the event of nuclear war. Kennedy's remarks prompted impassioned deliberations not only "about how best to protect the home, but also about whether the home *could* be protected against nuclear weapons, or even *should* be protected against nuclear weapons" (p. 9). In six chapters, Rose places these questions into social, cultural, political, and scientific contexts and challenges Cold War verities.

Rose begins by examining the connections between nuclear strategy and civil defense. Despite tremendous escalation of the arms race, U.S. officials and politicians never instituted coherent civil defense policies and repeatedly balked at the projected costs of a national fallout shelter program. Instead, they recommended evacuation plans or called for private funding of civil defense protections. At the same time, American citizens were deeply troubled by the moral implications of building shelters.

All of these individuals lived in a culture saturated with nuclear anxieties. Echoing Paul Boyer, Spencer Weart, Allen Winkler, and Margot Henriksen, Rose shows how fears of nuclear war permeated American popular discourse. Much more original is Rose's examination of the ethical dilemmas surrounding civil defense. With amazing consistency, advocates of fallout shelters were accused of cowardice and selfishness. The "shelter morality" debates were also infused with class tensions. Less affluent Americans complained that only the wealthy could afford to build private shelters. Unfortunately, Rose does not explore whether ethnic and racial lines also divided Americans in these discussions.

The civil defense debate affected institutions as well as individuals. Government agencies at all levels searched for ways to ensure the continuation of essential services after a nuclear strike. They adopted dispersal plans, amassed stockpiles of strategic materials, and constructed shelters. Most notably, the federal government built a secret bunker under the

Greenbrier resort in White Sulphur Springs, West Virginia. This enormous facility was designed to house the entire U.S. Congress plus 565 staff members. The civil defense fervor also reached the private sector and public education. Many companies placed microfilms of essential records in concrete-reinforced facilities. While school administrators searched for ways to fortify buildings without disrupting the learning process, teachers tried to inculcate civil defense without terrifying children.

Yet these herculean efforts did not result in a nationwide shelter program. Convinced that a nuclear war would destroy the global economy, most businessmen refused to allocate funds for civil defense. Unwilling to alter classroom styles and constrained by limited budgets, few school districts transformed educational facilities into community shelters. Doctors, atomic scientists, and military experts added to the chorus of criticism directed at the proponents of "limited" nuclear war.

In demonstrating the widespread rejection of civil defense programs, Rose makes a significant contribution to our understanding of the Cold War. Many historians have assumed that most Americans translated their anxieties about nuclear war into substantive preparations for atomic attack. The culture of postwar America certainly suggests that nuclear war and civil defense were deeply entrenched in the national consciousness. But Rose convincingly argues that even at the height of Cold War tensions, "Americans *talked* a great deal about fallout shelters, but relatively few Americans actually *built* fallout shelters" (p. 204). After the terror of the Cuban missile crisis gave way to the 1963 ban on atmospheric nuclear testing, Americans grew even more ambivalent about civil defense. In more recent years, not even the Reagan-era defense build-up or the Y2K problem convinced a majority of Americans of the possibility of surviving Armageddon. However one responds to such fatalism, Rose provides penetrating insights into the American psyche.

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ALEXANDER BLOOM, editor. *Long Time Gone: Sixties America Then and Now*. (Viewpoints on American Culture.) New York: Oxford University Press. 2001. Pp. xiv, 229. Cloth \$39.95, paper \$19.95.

Alexander Bloom, the coauthor of *"Takin' It to the Streets": A Sixties Reader*, has edited a new book on that era. Bloom's aim is twofold: "to dispel the myths and to try to construct both an accurate vision of the past and an understanding of its contemporary influences," and "to reestablish *this* sense of the 'sixties' experience" (p. 8). To do that, he has compiled a series of essays on various topics of the decade that obviously are intended to interest specialists in the era plus general readers and students. The editor's introduction is followed by ten essays on familiar themes such as civil rights, the new Left, the war at home, black

student activism, counterculture, gays, and the rebirth of feminism, and a more unusual look at the Freedom Singers and the Living Theater.

The collection presents a positive view of the 1960s, for most of the essays are written by former activists such as Julian Bond, Wini Breines, John D'Emilio, Sara Evans, and Barry Melton. A generation later, these folks are not going to turn around and tell readers that they were wrong, that the 1960s was a horrible decade that ruined lives and destroyed America. "The 'sixties' are the era that shaped me," D'Emilio admits. "I remember those times as thrilling, exhilarating, hopeful, exuberant. The universe cracked open and revealed to me endless possibilities" (p. 209).

Thus, Bloom's reader is a love child—and it is a fine one. Bond writes an essay on the civil rights struggle, and he engages readers by describing what he was doing in February 1960 when his friend Lonnie King came over with the *Atlanta Daily World*, whose headline read "Greensboro Students Sit-in for Third Day!" Then King uttered an invitation, a command: "Why don't we make it happen here?" (p. 11). Breines begins her essay by telling about in loco parentis rules at her campus in the late 1950s, how she stayed "illegally" in her boyfriend's room over Thanksgiving vacation, and how they both were placed on probation and almost expelled.

Some essays are more scholarly. Tom Wells is superb on how the Johnson and especially the Nixon administration misunderstood antiwar protesters. Based on numerous interviews, Wells notes that officials thought the protestors were "crazy people who had previous history in mental institutions," or they "were simply motivated by the desire to get out and raise hell." One Nixon aide felt protests were just "fashion . . . the latest hula hoop" (pp. 85–86). Christian Appy and Bloom pen another fine essay that uses cold logic and stimulating quotes to demolish myths about the Vietnam War. Tom Wicker thoughtfully traces the policies of Lyndon B. Johnson and explains how they led to the rise of contemporary conservatism, while Evans skillfully explains women's liberation so that young female students today will recognize that an older generation of women paved the way to equality.

Other essays are more autobiographical, and a gratifying example is by Melton, cofounder of the band Country Joe and the Fish, on his life in the counterculture. Now a public defender, Melton describes his own life as a California hippie. No Charlie Manson here, thank heavens; this essay would make sense to any open-minded reader, from Melton's devotion to folk music, his change to folk-rock, and life in a band of the movement. He is honest about drugs, sex, rock and roll, and the community that began and commercialism that resulted. "But for me," he concludes, "the 1960s I want to remember will always be that relatively small community of people in the San Francisco Bay Area who made and listened to the music . . . a host of creative people content to live with a ton of commitment and without a whole lot of money" (p. 156).

This book is a superb addition to the library on the 1960s—and it is a lucid and stimulating read.

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LARRY BERMAN. *No Peace, No Honor: Nixon, Kissinger, and Betrayal in Vietnam*. New York: Free Press. 2001. Pp. xv, 334. \$27.50.

Among all the controversies provoked by the Vietnam War, few have been more heated than that which has raged about the January 1973 peace agreements. Richard M. Nixon and Henry Kissinger have stoutly maintained that they established a foundation for peace with honor, only to be undercut by a vindictive Congress that bound their hands militarily and ruthlessly slashed U.S. military aid, leaving South Vietnam defenseless in the face of North Vietnamese aggression. Critics have claimed that Nixon and Kissinger sought nothing more than a cynical “decent interval” between U.S. withdrawal and the inevitable collapse of South Vietnam—and barely attained that.

The truth is much worse, Larry Berman argues in this stinging indictment of the secret negotiations that ended the war. However much they talked of peace, Nixon and Kissinger in fact viewed the January agreements as a means to continue the war. They were certain North Vietnam would violate the agreements, and they planned to retaliate with U.S. airpower. Nor did they have illusions of honor. They recognized that by leaving North Vietnamese troops in the South while withdrawing U.S. forces, they were signing South Vietnam’s death warrant. Indeed, in a variant of Daniel Ellsberg’s “stalemate machine” thesis, Berman concludes that Nixon sought to perpetuate an indefinite stalemate that would keep South Vietnam afloat until the end of his presidency, after which he could no longer be held responsible for what happened.

A political scientist and the author of two well-received books on Lyndon Johnson and Vietnam, Berman has been adept at exploiting newly released documents, and he does so again here. Totally in character, Nixon and Kissinger have sought to keep their papers from historians, but Berman gets around their restrictions by using other newly declassified U.S. sources, most importantly notes taken by Kissinger’s aides on his extensive secret discussions with North Vietnam. The book occasionally lapses into an exposé mode, and it is not footnoted in the usual fashion. Berman nevertheless uses this new documentation to craft a provocative analysis of the negotiations that led to the 1973 agreements.

He begins his tale of “diplomatic deception and public betrayal” (p. 10) with the Johnson administration’s opening of negotiations in Paris in May 1968 and concludes with the January 1973 agreement. He condemns the secrecy in which Nixon and Kissinger shrouded their diplomacy, and their repeated deceitfulness in telling one story to the North Vietnamese, another to the South Vietnamese, and often another to

the American people and Congress. Nixon clung stubbornly to Nguyen Van Thieu, Berman insists, mainly because he needed the recalcitrant South Vietnamese president to carry out his plan to continue the war. The apparent success of U.S. airpower in the May and December 1972 Linebacker operations confirmed Nixon in his strategy. He saw the 1973 peace agreement mainly as a way to continue the war, and he was convinced that the American people would support him when North Vietnam violated the terms.

The result was a “sham peace held together with a plan to deceive the American public with the rhetoric of American honor” (p. 261). There was little difference, Berman argues, between the National Liberation Front’s 1969 program and the 1973 agreement. All the latter accomplished was to secure the withdrawal of U.S. troops and the return of American prisoners of war. Thousands of people thus died “for very little, or simply while waiting for Thieu to give in because Nixon had allowed him to remain in office because Nixon believed that there was no acceptable American alternative to Thieu.” Thieu was betrayed not by Watergate, as Nixon and Kissinger have charged, but by the “men who kept him in power and made him sign the so-called peace agreement” (p. 246).

Berman demonstrates conclusively that Nixon and Kissinger knew the agreement would not bring peace. He produces convincing evidence that they intended to continue the war with airpower.

But much remains murky. The differences between Nixon and Kissinger are not always clear and what Nixon actually sought (if indeed he knew) is even less so. Did he perhaps delude himself that he could sustain South Vietnam’s independence? Did he really believe that he could continue the war until 1976, or, like Kissinger, was he merely seeking a decent interval? Or was he trying to shift blame to Congress for the fall of South Vietnam? The new evidence, however interesting, does not provide clear answers to these questions. Nor is the charge of betrayal—heretofore the mantra of the political right—altogether persuasive. By resisting negotiations and stubbornly clinging to power, Thieu helped prolong the war, and he was as much the agent of his own—and South Vietnam’s—destruction as a victim of Nixon—Kissinger perfidy.

A valuable addition to the literature, this book provides much new information and offers bold new interpretations of the complex and tortuous diplomatic path to the flawed agreements of January 1973. An authoritative account must still await the release of Nixon’s and Kissinger’s papers and additional sources from North Vietnam.

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ANDREA SLANE. *A Not So Foreign Affair: Fascism, Sexuality, and the Cultural Rhetoric of American Democracy*. Durham, N.C.: Duke University Press. 2001. Pp. x, 368. Cloth \$64.95, paper \$19.95.

The subject of Andrea Slane's recent book is one worthy of thoughtful study and consideration. She argues that the term "Nazi," and the layers of meaning that word conveys, are invoked by opponents on the Left and the Right in sociopolitical and cultural discourse. Nazi and fascist have become interchangeable words in a series of ideological debates in this country, especially in the realm of sexual politics. The quickest way to assume moral superiority is to label one's opponent a Nazi or a fascist. Women's rights advocates who believe that a woman should have control over her own body label anti-abortion activists and members of the Christian Right as Nazis. At the same time, pro-life advocates argue that doctors who conduct abortions are evil incarnate, perpetuating an "American Holocaust" (pp. 2-3); these so-called obstetricians are the true heirs of cold-blooded Nazi doctors like Josef Mengele. Likewise, debates over prayer in school and the separation of church and state have led to accusations that the Supreme Court is composed of godless Nazis intent on destroying the republic. Slane's point is a serious one. When both sides use the same label to demonize their opponent, serious debate over important issues can devolve into name-calling, opprobrium, and even violence.

Slane chose to use sexuality as the fulcrum of the discussion, for Nazis are regarded in diametrically opposite ways regarding sex. On the one hand, Nazis have been depicted in the popular culture as sexual libertines whose guiding spirit is the Marquis de Sade. On the other hand, they are depicted as sexual prudes; frigid beings afraid of emotion and passion. Both depictions represent conscious distortions of fact. Feminists have been caught in the crossfire, inspiring Rush Limbaugh and other reactionaries to label women who seek equality under the law as "Feminazis" (pp. 80-87). Also caught in the middle are gays and lesbians, who compare their victimization to the privations suffered by those who wore the pink triangle in Nazi concentration camps.

Beginning with an examination of the Nazi film *Die Goldene Stadt* (1942), Slane discusses how the Nazis used cinema to espouse their ideology. Adolf Hitler and his propaganda minister, Josef Goebbels, enjoyed movies and attempted to compete with Hollywood through the production of feature films at Ufa Studio. Films were intended for entertainment and as a means of educating the public about the values of the Nazi regime. Viewers were cautioned to look out for sexual predators, usually represented by groups the Nazis regarded as inferior: Jews, Gypsies, and Slavs. The Nazis promoted a conservative sexual agenda, arguing that appropriate sexual relations with appropriate partners within the confines of marriage promoted a healthy and stable nation. To have sex with someone not of the Aryan race, to engage in any number of sexual perversions, or to have sexual relations outside of marriage, they argued, weakened the state and could lead to the Reich's downfall.

The Hollywood and American journalistic version is

usually the exact opposite. A 1942 article, "Sex is a Nazi Weapon," made the American attitude toward the Nazi regime plain (p. 53): Nazis are sexual predators intent on turning all women into whores, nymphomaniacs, or breeding machines. The concept of *Lebensborn*, selecting men and women for their breeding capabilities to maintain the Master Race, directly contradicted the message that Nazi-produced films espoused. American popular culture has also depicted Nazis as closeted homosexuals, such as Peter O'Toole in *The Night of the Generals* (1966). Though he struts around in his smart uniform, barking commands, the cruelty of the Nazi officer derives from his efforts to suppress his own forbidden urges. Interestingly, both the Nazi filmmakers and their Hollywood counterparts cautioned against the sexual license of their opponents. In both variations, women are the ones most at risk.

Communism may be dead, but Nazism endures. Neo-Nazism, Nazi apologists, and the Skin Head movement reignited after the Cold War ended. State-sponsored racism reared its ugly head in the Balkans and, in the newly unified Germany, Gypsies were once again the target of violence. The Neo-Nazi novel *The Turner Diaries* (1978) galvanized Nazi sympathizers in this country and was Timothy McVeigh's inspiration for bombing the Alfred P. Murrah Federal Building in Oklahoma City. Slane argues that recent films whose stated intention was to caution against Neo-Nazism created an opposite effect. The Australian film *Romper Stomper* (1992) and Hollywood's *American History X* (1998) actually glorified the movement. Not only that, making films about the American Nazi Party, Ruby Ridge, David Koresh, and others gives their repellent stance legitimacy in some eyes.

The book is not without its problems, some of them serious. Slane uses terms like liberal and conservative as if they represented two cohesive, monolithic groups with consistent ideologies and ethos. Just as she argues that Nazi and fascist have been bandied about to the point that the terms are losing their meaning, the same is true of liberal and conservative. More problematic is the overuse of jargon. Rather than choosing clear, concise prose, Slane often gets lost in a jungle of imprecise language. Relying heavily on the work of social scientists and postmodernists like Michel Foucault and appropriating their convoluted rhetoric, Slane's important argument often bogs down. A more accessible book would not only reach a wider audience but might stimulate greater debate.

The discussion of *Ilsa, She Wolf of the SS* (1974), *The Damned* (1969), Lili Caviani's controversial examination of the "Stockholm Syndrome," *The Night Porter* (1974), while interesting, is overwritten and requires a broader context. Long regarded as a sick excuse for making a porn film, Slane correctly challenges scholars to take a more critical look at *Ilsa*. Omitted from the discussion were two significant Italian films that struggled with the implications of Benito Mussolini's fascism and the consequences of joining the Axis Powers:

Bernardo Bertolucci's epic *1900* (1976) and Lena Wertmüller's emotional rollercoaster Holocaust film *Seven Beauties* (1976). *Ilsa, The Night Porter*, and *Seven Beauties* complement each other in significant ways, asking difficult questions about what a person might do in order to survive. How much dignity would one sacrifice for a piece of bread, or to live one more day? Why did most of these significant and controversial films emerge in the mid-1970s? Also conspicuous in its absence from the discussion of the depiction of Nazism in popular culture is Mel Brooks's *The Producers* (1968), whose campy portrayal of the Third Reich used humor as a potent weapon.

Slane's book raises several important questions concerning the public's understanding and use of the terms fascism and Nazism. In the political arena, Nazi has replaced Communist/Commie/Red/Pinko as the preferred term of derision. During the Cold War, if one wanted to cast doubts on an opponent's worthiness or loyalty to America, all one had to do was say the individual sympathized with Communism; today one would probably call that person a Nazi. Images of Nazis have been constructed by journalists, novelists, and filmmakers, distorting reality and in some cases making Nazism attractive. A deeper investigation of the cultural creation of the Nazi stereotype is long overdue. Slane has written an interesting, though often infuriating, book. The subject is worthy of far greater attention, and she is to be commended for broaching it.

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WILLIAM N. ESKRIDGE, JR. *Gaylaw: Challenging the Apartheid of the Closet*. Cambridge: Harvard University Press. 1999. Pp. ix, 470. \$45.00.

William N. Eskridge, Jr., is a prolific legal scholar well known for his expertise in statutory and constitutional interpretation. He is also one of the nation's leading experts on gaylaw and gaylegal history, both Eskridge-coined terms. This book represents his most thorough treatment of these topics.

Like Gaul, the book is divided into three parts. Part one traces the historical development of "the closet" from 1880 to 1981. Building on the work of gay historians, and adding substantial original research, Eskridge tells the early story of the law's regulation of sexual minorities. His story is not the public history of reported cases and mainstream news accounts relied upon by many gay legal scholars. Eskridge visits the police stations of major cities to analyze their arrest records and consults the annals of the fledgling gay press of the 1950s for stories of police harassment. He details substantive changes in the law that expanded the definitions of illegal sexual conduct, and he notes some interesting correlations between these expanded definitions and the incidence of gay arrests (pp. 25–26). His treatment of these early laws is enough to make the book a must for scholars interested in legal

regulation of sexuality. Such scholars will especially treasure the three appendixes setting forth state and municipal laws, as well as statistics on arrests and immigration violations (pp. 328–84).

The "mutually protective closet" (p. 92) arose post-World War II, as Eskridge explains, in response to the fierce antihomosexual campaign carried out through military bans, government witch hunts, police abuse, gay bar closings, and literary censorship. By the 1960s, however, decisions by the Warren Court supporting civil rights for African Americans and due process guarantees for criminals made it possible for the gay community to contest the closet and to begin "coming out."

In part two, Eskridge explains why vestiges of the closet remain with us, despite magnificent progress regarding gay rights from the 1960s to the 1980s. He begins with a chapter on the infamous sodomy statute challenge from Georgia, *Bowers v. Hardwick* (1986), and critiques it for its inaccurate account of the history of criminal sodomy. In the next chapter he focuses on First Amendment arguments and makes an interesting but debatable point that sexual expression in private is entitled to first amendment protection (p. 177). Another chapter deals with the notion of gay equality as a plausible requirement of equal protection doctrine. *Romer v. Evans* which struck down Colorado's antigay constitutional amendment in 1996, provides support for Eskridge's arguments.

Part three turns to the future and bravely tackles some difficult issues that gay equality principles, once adopted, are likely to pose for liberal theory generally (e.g. the legal meaning of "consent" within sexual relationships with minors and sadomasochistic relationships; polygamy; multiple parenting arrangements). These are interesting topics, but my favorite portion of part three is Eskridge's development of the notion of "equality practice." He begins with the story of a young boy who goes fishing with his father and catches no fish. In response to the son's devastation, the father encourages the son to view their trip as "fishing practice" (p. 311). Many years later, the son, now an adult, comes out to his father, who does not immediately embrace this new knowledge about his son. The father explains that it will take him some time to adjust and the son remembers the lesson from "fishing practice." Just as it takes time to become an expert fisherman, gay equality will take some time. In Eskridge's hands, "equality practice" is shaped into a legal method. We, as a nation, need to practice gay equality before we attain it for real. The role of the courts, especially the Supreme Court, is to level the playing field for the opposing teams. Strong First Amendment protection for opposing points of view is crucial. But Eskridge adds a new twist. Just as a gay son and his fearful family need time to reach resolution, so do other opponents in this battle over gay rights. Honest, nonhateful, religious organizations, like families, deserve respect. So does the gay community. In a battle where each side seeks to trump the

other with its rights, courts should seek a middle ground that respects both sides. *Romer* provides a counterbalance to *Hardwick*. After *Romer*, lower federal and state courts can enter the skirmish, and give the respect to the gay community that *Hardwick* had denied. The Supreme Court should probably stay out of the battle for a while. Otherwise, it might upset the balance. There will be gay victories and losses. But, eventually, "equality practice" should lead to true gay equality. Eskridge offers an optimistic and pragmatic view of U.S. courts as agents of incremental change. There is nothing terribly new here. But the idea that the Supreme Court is preserving the "practice area" is an engaging concept for those of us still fishing for equal rights.

There is much of value in this book: history, theory, ethics, and narrative. Of greatest value is the history. If you choose one title to read on gaylegal history, this should be the one.

PATRICIA A. CAIN
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KIRK JEFFREY. *Machines in Our Hearts: The Cardiac Pacemaker, the Implantable Defibrillator, and American Health Care*. Baltimore: Johns Hopkins University Press. 2001. Pp. xiii, 370. \$48.00.

In the mid-1920s, Mark C. Lidwill, a physician-anesthetist from Sydney, Australia, was frustrated by his inability to revive stillborn infants by employing standard methods. At a meeting in 1929, he reported that several years earlier he had revived one infant after he applied an electrically powered invention to the child's heart. He placed one of two poles attached to the machine on a pad saturated with a salt solution on the child's arm and the other pole, a needle insulated except for its tip, into the lower chamber of the heart. Electrical stimulation began when the device was plugged into a wall socket. After ten minutes, the infant's heart was beating, his life saved by Lidwill's crude heart pacer. The concern of an anesthetist to keep patients alive under threatening circumstances in the operating room brought about a new era of using heart pacers to restore or correct the human heart's dependence on its beating mechanism. Surgeons doing open-heart procedures and engineers, who understood the new technology of electrical pacing, would become the leading inventors and promoters of ever-improving pacers beginning in the post-World War II period.

In 1958, the first pacemaker was implanted into a human body. By this time, physicians' acceptance of penetrating the human heart as an acceptable form of treatment for heart failure had been achieved, overcoming a taboo that had existed for millennia. The patient's cooperation in wearing a device to control the heartbeat grew out of the public's awareness of the remarkable operations and devices used to save lives during World War II. With the new frame of reference for acceptable medical treatment and the patient's ability to endure complex procedures and the limita-

tions these placed on life, it remained for surgeons and engineers to perfect the prescription, size, shape, and composition of the heart pacer. Many contributed to this enterprise. Among pioneering physicians were Paul Zoll and C. Wilton Lillehei. The engineers they worked with included Earl Bakken, founder of Medtronic, which developed the world's first implantable pacemaker and whose company became the largest manufacturer of medical devices in the twentieth century. Wilson Greatbatch, whose name is most commonly associated with heart pacers, received a patent that in 1983 was selected as one of two major engineering contributions to society during the previous half-century. Greatbatch established a series of companies, which manufacture most of the batteries for pacemakers to the present day.

The history of the pacemaker and later, the defibrillator, is discussed in considerable detail in Kirk Jeffrey's book. The impact on and the role of American health care in adopting ever more technical devices to treat diseases and improve the quality of life is a subtheme that will be of value to many readers. For those who lack knowledge of technology or medicine, Jeffrey is careful to explain what is needed before proceeding. (For those who desire more information, there are a number of websites to consult, especially for those seeking support as heart pacer patients.) Pacemakers have become a big business and in doing so, Jeffrey concludes that they "have met doctors' and patients' needs, but at the same time they have opened new possibilities and helped to create new desires for more sophisticated heart-rhythm management devices" (p. 288). Jeffrey and others wonder how these treatments should be allocated and paid for. The historian's role in analyzing the many social and cultural factors that impinge on medical treatment, and its cost, is crucial to all of us, and this book provides much stimulation for further thought. One might even begin by wondering why the cost of medical care is always so carefully scrutinized, while military and other expensive endeavors are seen as better bargains.

AUDREY B. DAVIS
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JORDAN GOODMAN and VIVIEN WALSH. *The Story of Taxol: Nature and Politics in the Pursuit of an Anti-cancer Drug*. New York: Cambridge University Press. 2001. Pp. xi, 282. \$27.95.

As the title indicates, this book tells the story of the discovery and introduction into therapy of Taxol (generically known as paclitaxel), a remarkable drug that has proven effective in the treatment of breast and ovarian cancer. The authors, Jordan Goodman and Vivien Walsh, succinctly summarize the scope of the work thus: "This book is the history of taxol, the anti-cancer drug, and of the tree, *Taxus brevifolia*, the Pacific yew, from which it derives. Our story begins in the 1960s when Western science 'discovered' the exis-

tence of taxol in the yew bark and ends, for our purposes, in the 1990s when corporate capitalism took over the drug and abandoned the tree" (p. 1).

Taxol was discovered as the result of a joint program, begun in 1960, of the National Cancer Institute (NCI) and the United States Department of Agriculture involving the procurement and screening of plant products for anticancer activity. Although the Pacific yew was procured as a potential source of drugs in the early years of the program, it was not until 1984 that Taxol received its first clinical trials. Goodman and Walsh provide a thorough and fascinating account of the scientific history of the drug, and also of the political and economic factors involved in its development. Because the only source of the drug at first was the Pacific yew, an important component of the old-growth forests of the Pacific Northwest, the drug became embroiled in the environmental controversy over the preservation of these forests. The tree, because of the value of the drug derived from it, also came to serve as an example of the importance of maintaining biodiversity. Concerns about the supply of the tree became moot when Bristol-Myers Squibb, which manufactured the drug under an agreement with the NCI, adopted a semisynthetic method of production that did not make use of the yew.

The story of Taxol is a significant one that has received little attention from historians, and this book fills a gap in the literature. It is carefully researched, drawing upon a host of published and unpublished sources, as well as interviews with some of the key participants. On the whole, the book is also well written. The authors do not shy away from the technical aspects of the story, which is to be commended for a subject that so heavily involves science and technology. However, this approach will make the book difficult reading in some places for those whose knowledge of science is quite limited. The rewards are well worth the effort.

It was not the chemical and biological terms that concerned this reviewer. I must confess to some trepidation when the introductory discussion of actor-network theory put forth such notions as "enrolment," "translation," and "obligatory points of passage" (p. 4). The brief definitions provided for these concepts did little to ease my mind. The authors claim that these ideas were of great value to them in researching and writing the book. Goodman and Walsh also note that network theory is "implicit throughout the text, and occasionally explicit" (p. 4). I found little explicit discussion of the theory in the book, and perhaps my unfamiliarity with the concepts involved prevented me from seeing any implicit connections. Thankfully, the work was largely free of any jargon associated with network theory, and if the use of this theory by the authors contributed to the overall success of the book, that is certainly all to the good.

I was a little disappointed in the conclusion of the book. It seemed to end rather abruptly, without any significant summing up or final analysis. This is a minor

quibble, however, about what is overall an excellent book. It should be of interest not only to historians of science and medicine, but also to environmental and business historians, and in fact to anyone who would like to learn more about the development of this important therapeutic agent.

JOHN PARASCANDOLA
National Library of Medicine

CARIBBEAN AND LATIN AMERICA

LOUIS A. PÉREZ, JR. *Winds of Change: Hurricanes and the Transformation of Nineteenth-Century Cuba*. Chapel Hill: University of North Carolina Press. 2001. Pp. x, 199. Cloth \$49.95, paper \$17.95.

How the hurricanes of 1842, 1844, and 1846 affected the socioeconomic and political development of Cuba is the topic of Louis A. Pérez, Jr.'s study. Employing rich archival material from Cuba and Spain, travelers' accounts as well as popular literary forms, Pérez successfully places this climatological phenomenon at the center of the island's history, culture, and its peoples' psychology. He argues that the hurricanes of the mid-nineteenth century redefined the position of social classes within colonial society as well as the colony's relationship with Spain. More important, these storms permanently changed some of the dominant characteristics of the economy, particularly its land tenure forms and its labor and production systems. Their legacy shaped the collective memory of Cubans well into the twentieth century.

The first part of the book includes a comparative examination of how hurricanes became an unpredictable factor that affected the Caribbean region's patterns of colonization and development by European settlers. Using their records and travel accounts, Pérez is able to reconstruct the destructive nature of Caribbean hurricanes during the seventeenth and eighteenth centuries as well as how Europeans responded to them.

The middle chapters of the book explore why the hurricanes of the 1840s were so destructive in Cuba, and how they altered Cuban society, economy, and politics. Tracing Cuban history between 1820 and 1844, Pérez concludes that the island's coffee, sugar, tobacco, and cattle industries expanded to such a degree that they brought Cuba unprecedented development and prosperity. Prosperity led to an increase in the population and engendered the island's dependence on African slavery. Cuban prosperity was also reflected in the way rural and urban elites in the western part of the island, particularly those living in Havana, luxuriously displayed their wealth before the first storm hit the island in 1842.

Pérez argues that the island's location in the Caribbean and the month in which the more damaging hurricanes of 1844 and 1846 battered Cuba help explain their horrific impact. The most destructive hurricanes to hit Cuba did so between September and

November. Those of the 1840s made landfall in October on the southwestern coast, developing enormous power as they moved northward. These storms ravaged the island because its physiography included some two hundred harbors, bays, and inlets. Cubans also had established an extensive number of settlements on the island's southern and northern coasts. Even nineteenth-century meteorology could not prepare Cubans for the storms, as it was incapable of judging either the speed and the magnitude of the hurricanes or the power of the rain and wind, according to Pérez.

How the storms of the 1840s affected Cuban society, economy, and politics is discussed in the last half of the book. Arriving during the critical stages of the production cycles of sugar, coffee, and other crops and staples, the hurricanes impoverished all sectors and classes of society. Whites lost their property, lifestyle, and jobs. White rural workers became vagrants in the towns and cities, while urban and rural elites experienced psychological trauma. Although white Cubans suffered, African slaves were the ultimate victims of the hurricanes. Not only did they lose their few material possessions but also the plots on which they grew food.

The economic changes caused by the hurricanes reveal why sugar production expanded after 1846. Pérez discovered that the hurricanes destroyed the coffee industry to such an extent that land, labor, and capital formerly used in that industry were reallocated and employed to develop and expand the sugar industry. This shift in resources also occurred as Brazilian and East Indian coffee growers began to seize much of the international market. Pérez's interpretation questions those proposed by Jeffrey Sachs, Felipe Larraín, and Richard J. Salvucci, who argue that the ascendancy of sugar in Cuba was caused by the decision among coffee producers themselves to leave a comparatively unprofitable industry in order to venture into one that yielded more income. Spanish commercial policy and high inflation also informed their decision to remove themselves from the coffee industry. Nevertheless, the primary losers in this transformation were the slaves who had worked on the coffee estates. There, the labor regimen was significantly less harsh and demanding than that of sugar plantations, as is apparent when the mortality rate of coffee estate slaves is compared to the rate for sugar plantation slaves.

The hurricanes strained Cuba's relationship with Spain. Colonial officials in Cuba and in Spain proved incapable of assisting the thousands of victims who petitioned the government for aid. The lack of resources and poor communication between colonial officials and Spain prompted Cubans to request assistance from the United States. The United States immediately responded and helped to mitigate the material loss Cubans suffered. More important, U.S. aid moved Cuba toward its economic sphere of influence, to the dismay of the officials in Spain.

Pérez concludes by placing his study within the

broader literature on the social and cultural consequences of natural disasters such as hurricanes. In doing so, not only does he support his overall argument that the mid-nineteenth-century storms altered social, political, and economic structures and processes in Cuba, but he also shows that they informed emerging Cuban concepts of nationhood and identity. The latter is better understood in Pérez's discussion of how hurricanes have been incorporated into Cuban colloquialism and historical and contemporary psychology.

Like his other works, this study contributes to our understanding about Cuban concepts of gender, race, class, culture, identity, and ecology. Pérez's contribution would have been stronger if he had informed us what ethnic group the majority of Cubans belonged to, particularly those who lived in Havana, who petitioned Spain for assistance during the aftermath of the hurricanes. If they were predominately Afro-Cuban, did race influence the haphazard nature of Spanish policy toward Cuba? Nevertheless, Pérez continues to narrate and interpret the hopes, aspirations, and expectations of Cubans who lived within the broader historical process of the nineteenth century. As a result, this book ought to be read in both undergraduate and graduate-level Latin American and Caribbean history and geography courses.

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JEREMY BASKES. *Indians, Merchants, and Markets: A Reinterpretation of the Repartimiento and Spanish-Indian Economic Relations in Colonial Oaxaca, 1750–1821*. Stanford: Stanford University Press. 2000. Pp. 305. \$60.00.

The practice of forced consumption, or the *repartimiento de mercancías*, has drawn a lot of scholarly attention in Latin America. Noticed and condemned by Spanish critics of their own colonial rule as early as the 1740s, modern writers took up the subject in the 1970s and 1980s as a telling example of the kind of draconian abuse present in colonial regimes and to demonstrate its role in promoting Indian rebellion. In these studies, local crown officials (*corregidores*) in cahoots with wholesale merchants in Lima or Mexico City repeatedly hauled to Indian villages an array of goods including cloth, iron hoes, mules, and shears, which they forced reluctant consumers to purchase at exorbitant prices and onerous terms. This was done with the assistance of village leaders (*kurakas* and *caciques*) who then collaborated with the *corregidor* in the collection of installment payments, since the goods were advanced on credit. The combination of pushy merchants together with the political and legal means to collect back payment made especially effective the practice of "forced consumption," particularly so in eighteenth-century Peru.

Jeremy Baskes takes up this subject for colonial Oaxaca, in southeastern Mexico, in a deeply researched microhistory whose data are strained through

the colander of neoclassical economic theory. In this story, merchants also provided goods, but mainly capital, to the *corregidores*, who in turn extended small amounts of money to a large number of individual peasant producers of cochineal, the highly valued vermilion dye. In a typical example, the *corregidor* or his agents advanced the peasant producer some ten pesos, with the understanding that in six or seven months he would deliver a pound of the dried and compacted insect shells for every 1.5 pesos advanced. Having obtained the cochineal at a peso and a half a pound, the *corregidor* then sold it, through the merchant, for prices that varied from around two pesos to as much as four or five when demand was strong. In some cases, the Oaxacan *corregidor*, as in Peru, also advanced goods and mules on credit to the Indian producers at well beyond the market price.

Rather than "forced consumption," Baskes sees the Oaxacan *repartimiento* as a more or less benign credit system in which the *corregidor* naturally required a decent profit to compensate for the risks involved in peasant production and uncertain weather. For their part, the Indian beneficiaries of credit made rational choices, acquired tools and mules to assist production, and preferred to buy goods on installment rather than spend their scant pesos on cash purchases. They had agency. This, of course, is a brutal summary of Baskes's detailed and complex argument. The book makes a useful contribution to the colonial history of both Mexico and Peru by turning up new detail and by demonstrating the utility of applying sophisticated economic theory to recalcitrant data.

We might ask, however, why such similar data and apparently similar practices in Peru and Mexico yield such different interpretations? Perhaps in the 1970s scholars were more inclined to seek and find colonial repression than in the new millennium when the market, if it does not always smooth the way to global bliss, is widely presumed to allocate resources in the most rational way. I certainly do not want to impute such unconscious motives to Baskes; perhaps we simply ask different questions now. Another explanation may be that most of the Peruvian studies are descriptive and rely heavily on contemporary opinion and literary evidence, while Baskes's Oaxacan study arranges a great deal of quantitative material and examines it in the sophisticated light of transactions-cost theory and the concepts of cross-cultural trade, along with the usual tools of neoclassical economic theory. Finally, the difference may be explained by the real differences between eighteenth-century Peru and Mexico. The colonial state seems less willing or able to hold to account the abusive local governors in the far reaches of highland Peru than in Mexico. Moreover, in Peru, the Indian *kurakas* held on to their power much longer than in Mexico and were often zealous collaborators of trade-driving *corregidores*; in Baskes's Oaxacan study, the *caciques* are almost entirely absent.

However impressive and provocative Baskes's innovative evaluation of the Mexican material is, he may

overreach in his effort to challenge the current interpretations of the *repartimiento de mercancías* across the board of colonial Spanish America; but his keen analysis of the Oaxacan case requires any future scholar of this subject to take his work into account.

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THOMAS BENJAMIN. *La Revolución: Mexico's Great Revolution as Memory, Myth, and History*. Austin: University of Texas Press. 2000. Pp. xi, 237. Cloth \$40.00, paper \$18.95.

Through this study of culture and intellect, Thomas Benjamin provides us with a highly insightful and useful examination of the roles of monuments and rituals and the meaning of history in the construction of a dominant collective memory in twentieth-century Mexico. In the process, he examines myths, *corridos*, the pantheon of heroes, the plastic arts, sporting events, and holidays. He underscores the dramatic changes in Mexican culture in the years since 1910.

Benjamin argues that histories written during and immediately after the Mexican Revolution were varied in perspective, but the authors, reflecting the tenor of the times, were polemical in their arguments. The old regime, Maderistas, Carrancistas, and Obregonistas each had supporters who in turn attacked rival groups. By the early 1920s, the most erudite—Francisco Bulnes, Manuel Calero, Emilio Rabasa, and José López Portillo—stood out as the foremost figures of the postrevolutionary historical dialogue. Their disputes were as profound as those of the causes with which they sympathized. Benjamin points out that for many of them, books served as substitutes for weapons. By the 1930s, however, a new school of historians was emerging. While still moved by ideology, José T. Meléndez and Luis Chávez Orozco anticipated the future. Meléndez published two edited volumes to complete the first comprehensive history of the revolution. Meanwhile, Chávez Orozco, one of the more salient figures in this new group, openly complained about the personalist objectives of his predecessors and moved toward the development of documented essays.

At this juncture, however, the government began to take a greater interest in the historical image of revolutionary causes, heroes, and antiheroes. President Lázaro Cárdenas underscored the importance of popular heroes such as Emiliano Zapata for national cohesion. He saw, and wanted the masses to see, Francisco I. Madero, Zapata, and the others as men pursuing just causes, men of whom Mexicans could be proud. To further that aim, the Cárdenas administration approved the publication of Meléndez's work by the government press. In 1949, the rightist government of Miguel Alemán went further: it sponsored a national history book prize, and a distinguished panel of historians awarded Alberto Morales Jiménez the first

prize. Morales Jiménez argued, as had the surprised first secretary of development in the Madero administration forty-eight years earlier, that the revolution had been agrarian in nature, a struggle, in effect, for social justice. The government later placed the book in every school in the nation as a basic reference. In 1953, the Adolfo Ruiz Cortines administration took the next step when it created the Instituto Nacional de Estudios Históricos de la Revolución Mexicana, a still prolific publisher of revolutionary histories. Benjamin argues that this was the beginning of the golden age of the Mexican Revolution's "official" histories.

Meanwhile, the rich practices of Mexican mythmaking and the construction of public monuments also contributed to the creation of postrevolutionary memory. During the course of the twentieth century, governments sponsored the introduction of revolutionary heroes to local sculpture, art, ceremonies, and rituals, where they joined locally revered saints, the Virgin, and Christ as objects of veneration, or at least some regard. The author points to the government-financed shrines devoted to Álvaro Obregón and Zapata as centers of reverence for personages seen as epitomes of virtue.

Benjamin emphasizes a top-down process of cultural production. The government has indeed played a large and obvious role in the making of history and the plastic arts. But it is not the most interesting force. While Mexican historians choose their themes and reach their conclusions at least as independently as their American counterparts, an equally dynamic situation prevails in music and art. In contrast to the partially independent murals of Diego Rivera, José Clemente Orozco, and others whose work was subsidized by the government, input often came from sources that are difficult to study. Witness the wall paintings, campaign signs, street and dormitory banners, graffiti, and alternative *corridos* that have abounded in Mexico for the last eighty years. The author frequently mentions the officially accepted *corridos* and murals in the text, but the unauthorized pro-Villa ballads like those sung in the Tenampa Bar in the 1960s; the posters linking Demetrio Vallejo, Lucio Cabañas, Ruben Jaramillo, Genaro Vazquez, and SubComandante Marcos with Zapata, Cuauhtémoc, and the ever more saint-like Ernesto "Che" Guevara; and the prevalence of local oral traditions all require more study. University students, *campesinos*, villagers, and the urban working class have exercised these cultural initiatives for decades. Pancho Villa and Calixto Contreras were genuine heroes in much of Durango and parts of Chihuahua long before the government adopted them.

These complaints, however, are rendered insignificant when we remember that Benjamin has placed the entire revolutionary cultural milieu on the table. This book, critical of the government's role in the production of revolutionary culture, is a marvelous beginning. We need more studies of modern Mexican culture at this level of erudition in order to come to grips with

the questions that are inevitably raised by any good study. This book is essential reading for scholars of the revolution and modern Mexico.

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STEPHEN R. NIBLO. *Mexico in the 1940s: Modernity, Politics, and Corruption*. (Latin American Silhouettes: Studies in History and Culture.) Wilmington, Del.: Scholarly Resources. 1999. Pp. xxv, 408. \$55.00.

In this book, which has many positive as well as problematic aspects, Stephen R. Niblo purports to trace corruption in Mexico to the ten-year period beginning in December 1940, when capitalist-oriented presidents ended the honest government supposedly instituted in the period 1934–1940 by socialist-oriented President Lázaro Cárdenas. By omission, Niblo claims implicitly that Mexico did not suffer from massive corruption until the 1940s, when he sees the country's first real push for industrialization take place. Niblo thus condemns industrialization as bringing with it the themes of his subtitle: "modernity, politics, and corruption." The author ignores the many historical roots of corruption in Mexico going back centuries.

Niblo states that this book is intended to complement his earlier *War, Diplomacy, and Development: The United States and Mexico, 1938–1954* (1995), which examines diplomatic, military, economic, and business relations. In considering the two books as the first and second volumes in a closely related series, we find that Niblo belies his promise in the first volume (p. xiv) to explore in this second volume the domestic political activities that "brought an end to the economic nationalism" of President Cárdenas. Rather, in both volumes Niblo focuses on Mexico's industrial push as if it came only from the United States. Neither book studies major aspects of Mexico's internally generated economic development. Indeed, Niblo seems generally unaware of Cárdenas's pre-1940 role in laying the basis for Mexico's industrial policy of the 1940s and 1950s.

Economic interpretations by U.S. academics are almost absent from Niblo's two books, and no scholarly Mexican economists (such as Enrique Cárdenas, Víctor L. Urquidí, or Leopoldo Solís) are even mentioned in the bibliography of the book under review, let alone discussed in the text. These are important omissions, because Niblo writes about political economy but tries to reveal complexity only from "political" and "social" perspectives and not domestic economic perspective. Niblo's Mexico of the 1940s is one without the positive influence of *braceros* (laborers) moving between Mexico and the United States and where the dramatic rise of the middle class goes unnoticed.

How did Niblo "tap the memories" of the 1940s? He neither tells us with whom he spoke at the end of the 1950s, nor does he reveal whether or not he recorded his conversations, which became so vivid for him that he felt that he had experienced first-hand living in the

1940s. Given his stated concern about oral evidence, Niblo does not seem to understand how historians can record testimony in order to pit the life justifications, selective memory, and vantage point of one person against those of another in order to aid our understanding of the complexity of events. The idea that "corruption" is complex and involves different dimensions and levels is not highlighted by Niblo, who quotes selectively from Ramón Beteta (ideologue for both Cárdenas and Miguel Alemán) and misses Beteta's important distinction between the unethical and the illegal.

Further, Niblo seems unaware of General Juan Andreu Almazán's important testimony (*Revista Mexicana de Ciencia Política* 20:77 [1974]: 59–65) that, as Minister of Communications and public works in 1930, he joined together with President Pascual Ortiz Rubio (1930–1932), Secretary of War Joaquín Amaro, Secretary of the Treasury Luis Montes de Oca, and Governor of Michoacán Lázaro Cárdenas to form a private company to develop Acapulco in the state of Guerrero. The company acquired (by devious means) the property rights to the choice communal land on the Bay of Acapulco, where they built twenty bungalows to rent to the public. Once the partners realized in 1932 that such a deal could harm any future political career, they turned over their shares to Almazán. When Almazán lost the election of 1940 to Manuel Ávila Camacho, he did not choose the route of military rebellion (as had been the case previously in Mexican history) for one main reason. He had a convenient and profitable place to retire peacefully—"his" Hotel Papagayo. Therefore, Ávila Camacho, grateful for peace, did not challenge Almazán's dubious acquisition of the hotel and land. Settled in Acapulco, Almazán contributed to Mexico's economic development in the 1940s by using that choice land to build the then-huge Hotel Papagayo, thus establishing the basis for Mexico's modern coastal tourist boom.

This case of Almazán and Manuel Ávila Camacho calls into question Niblo's blanket condemnation of "corruption." Indeed, the idea of corruption is complex and involves what Woodrow Borah and I have defined as a "primitive form of capital accumulation." At least the capital "created" by Almazán was invested in Mexico and did open the country to income from foreign tourism. Since then, many wealthy Mexicans have become famous for accumulating Mexican capital outside of the country to the detriment of Mexico's development.

But enough of the book's problems; its successes are many. Niblo taps into Mexican archives to reveal interesting insights about how government officials and private entrepreneurs dealt with each other. His best chapter is his first which gives a warm view of life in Mexico in the 1940s, from art to film to popular customs. Niblo makes a major contribution by exposing the nefarious activities of Maximino Ávila Camacho, brother of the president. The story of Maximino (minister of communications and public works and

lord of the State of Puebla) reminds us that many major Mexican leaders have been "protected" from direct charges of corruption. Unfortunately, Niblo's account of how Maximino's henchmen botched the assassination of one of the governor's critics understates the evils of political corruption that Niblo seeks to expose. Never mind: Niblo tells with verve the hilarious story of how the intended victim and his new bride finally escaped the clutches of Maximino, their honeymoon only temporarily postponed.

For Niblo, however, the real honeymoon in Mexican history comes in the era of his hero, Cárdenas. The Cárdenas administration, he argues, motivated government officials to "work for the public good and to set aside age-old practices of personal enrichment" (p. 256). In contrast, according to Niblo, corruption seems to go hand-in-hand only with "capitalists," presumably putting Mexico in limbo between the "personal honesty" of President Porfirio Díaz (1876–1911) and Cárdenas.

Niblo's overreliance on the memos of U.S. diplomats is problematic. He seems to think that they are more reliable than the views of Mexicans themselves. In contrast, I have found that each has a different "truth," which, regardless of the reality of the moment, may interact with events to change the long-term course of history.

In conclusion, Niblo has yet to fulfill his plan to write about Mexico's domestic economic policy so that readers can properly weigh his claim that the United States has distorted Mexican economic and political policy making.

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JOSÉ R. DEUSTUA. *The Bewitchment of Silver: The Social Economy of Mining in Nineteenth-Century Peru*. (Monographs in International Studies. Latin American Series, number 31.) Athens, Ohio: Ohio University Center for International Studies. 2000. Pp. xvi, 290. \$28.00.

Like petroleum in the second half of the twentieth century, precious metals lubricated the wheels of international capitalism between the early sixteenth and the late nineteenth centuries. Silver was always more important in the process than gold (despite the 1:16 differential in their values in terms of weight for most of the period), and most of it came from Spanish America. In the sixteenth and seventeenth centuries, the mines of the modern republics of Peru and Bolivia were the principal producers; in the eighteenth century, primacy passed to Mexico, which by 1800 was responsible for two-thirds of the annual Spanish American output of around 35 million pesos (or dollars), but mining continued to underpin both the internal and external economies of the Andean region, within which the mines of Lower Peru—notably Cerro de Pasco (better known in the twentieth century for its

copper)—had become more important than the legendary Potosí in Upper Peru/Bolivia.

In his earlier work, especially *La minería y la iniciación de la república, 1820–1840* (1986), José R. Deustua has tracked the recovery of silver mining in republican Peru from the dislocation of the second and third decades of the nineteenth century (when the flight of labor and capital during the War of Independence and flooding at Cerro de Pasco combined to reverse the expansion of the last half-century of the colonial period). By the early 1840s, as he reminds us in the second chapter of this new study, registered output of silver at the dominant Cerro de Pasco had surpassed the levels achieved immediately before independence; thereafter, this center's production leveled out at around 200,000 marcs (88,000 kilograms) a year, before declining significantly during the last two decades of the century. However, increased output at other centers raised Peru's total silver output in 1898 to its highest recorded level—717,000 marcs (315,000 kilograms)—albeit in a period when the international value of silver fell substantially. The denouement for the independent silver producers of Peru came at the turn of the century, when they sold out to the foreign-owned Cerro de Pasco Copper Corporation, which quickly turned the center of the old colonial town of Pasco into a vast opencast mine, producing above all copper—but also lead, zinc, other industrial metals, and of course silver—for the smelting complex of La Oroya, linked by rail to Cerro de Pasco by 1904.

The present study provides a coherent overview of both the historiographical context for the study of mining in Peru between independence (1824) and the end of the nineteenth century, and the principal curves in production of not only precious but also industrial metals, principally tin and copper. It utilizes a combination of disparate sources, including fragmentary data on the registration and minting of gold and silver, and on exports, to demonstrate that, despite its diminishing importance in comparative international terms from mid-century, mining, particularly of silver, remained the key determinant of Peru's social economy. This conclusion provides the platform for the analysis in the three middle chapters of the book on "Mines, Mineowners, and Mineworkers," "Transportation and Trade," and "Railroads and Muleteering." The first paints a rather indistinct portrait of mineowners, perhaps not surprisingly given the relative paucity of scholarly research into the activities of Peru's non-metropolitan elite, but provides a persuasive explanation of the economic and social linkages of mining settlements with surrounding agrarian and pastoral communities, not least through the recruitment of labor. The discussion of transportation—by the teams of mules and llamas until the 1870s, and thereafter increasingly by the growing network of railways linking the interior of Peru with a series of Pacific ports—is impressive. It is shown that muleteering did not disappear but was increasingly subordinated to the railroad, particularly from the late 1890s, "as copper mining

transcended the social and economic rules set by the silver-mining industry" (p. 173). A concluding chapter provides a succinct discussion of "Mining and National Development," which restates the author's belief about the enduring importance of the mining sector for the economy and society of Peru throughout the nineteenth century. Overall, the study is clear (despite a slight rigidity in the use of English), direct, well-informed, and—given its empirical approach—thankfully free from overconceptualization.

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MARIANO BEN PLOTKIN. *Freud in the Pampas: The Emergence and Development of a Psychoanalytic Culture in Argentina*. Stanford: Stanford University Press. 2001. Pp. xiii, 314. Cloth \$60.00, paper \$24.95.

In recent decades, psychoanalysis has not fared well in most parts of the world. Heavily organic and biologic approaches to the human mind and psyche have secured hegemony over psychoanalytic probing of the largely concealed inner psyche. Whereas psychoanalytic insights continue to garner limited interest in study of the humanities, they have hardly represented a growth industry in intellectual and academic discourse. Argentina has been different, and this book is the first detailed study of how clinical work, literary and cultural movements, and even intellectual and political discourse have been and continue to be profoundly shaped by psychoanalytic premises.

Mariano Ben Plotkin details how psychoanalysis emerged as a significant force owing to a crisis of positivism that often centered on the problematic hereditary degeneracy paradigm in Argentine psychiatry during the late 1920s and early 1930s. Stressing more than surface symptoms, psychoanalysis accompanied other elements in what might be called a late-coming social and economic modernization of Argentine life. It helped to explain—and at a seemingly complex level—what mental moods were and how they evolved. Upper-class and educated elites were particularly drawn to popularized versions of Freudian theories of the dynamic inner psyche. As well, psychoanalysis appealed to some on the political Left, but it also had resonance to others on the Right within the military and the Roman Catholic Church.

By 1942, the Argentine Psychoanalytic Association (APA) was founded. Spanish-speaking emigrés like Maria Langer and Angel Garma, who had been trained in Vienna and Berlin and who had escaped as the Holocaust emerged, played central roles in establishing it. But most of the founders were locals from Buenos Aires, and they adapted psychoanalysis to the particulars of Argentine culture. A wealthy psychoanalytic patient created a foundation to fund APA training, and by the late 1940s Argentina had become the major psychoanalytic training center in Latin America. Society leaders were judicious, after World War II, not to inflame a hostile Perón government.

With important exceptions, they stayed out of the tug and pull of Argentine politics and thus maintained a measure of autonomy. Most eagerly embraced the theories of Melanie Klein and her British-based psychoanalytic movement, with its stress on subjective "object relations" between the very young infant and small fragments of the world he/she perceived. But on translating British Kleinians into Spanish and elaborating Kleinian formulations, the APA founding generation tended to give object relations theory several significant homegrown twists.

With a secure institutional and doctrinal base, psychoanalysis quickly proliferated throughout Argentine society. Psychoanalytically informed group therapy and psychodrama arrived in the 1950s, pressing Sigmund Freud's "science" beyond its professional and middle-class margins. An APA founder, Arnaldo Rasovsky, routinely appeared on radio and television and in popular magazines with messages that combined psychoanalytic modernity and starkness (e.g. he proposed a proclivity in parents to hurt children) with conservative prescriptions for women to fulfill their missions primarily through motherhood. By the 1960s, another APA founder, Enrique Pinchón Rivière, wrote a regular newspaper column on problems of adaptation to the environment in everyday life that resonated with a Marxist-sympathizing Left. Eva Giberti's *Escuela para Padres* (1968) on psychoanalytically informed child rearing techniques became a best seller. Gradually psychoanalytically informed expression became a primary way to talk of one's discontentedness, neuroses, and therapeutic needs. Even more than men, women embraced this means to communicate their feelings and often discovered that their personal psychoanalytically guided therapist was in many ways replacing the influence of their priest.

Plotkin's narrative of the process of psychoanalytic diffusion in Argentine culture underscores the considerable influence of the psychology profession. During the 1960s and 1970s, there was a major proliferation of psychology programs at Argentine universities; most turned out psychoanalytically informed clinical psychologists. (By 1995, Argentina may have had more practicing psychologists proportionate to its population than anywhere else in the world.) Because clinical psychologists (mostly women) charged lower fees than psychoanalysts and psychiatrists (mostly men) and often worked in public hospitals and other public agencies, they had become the primary professional agents for the dissemination of psychoanalytically based therapy. Discriminated against within the APA, no few of them joined with leftist intellectuals to embrace Lacanian psychoanalytic theory outside APA operations. Home-grown variants of Lacanianism and Kleinianism now contested each other throughout Argentine culture.

Plotkin's final chapters concern psychoanalytic politics. The Argentine New Left was disillusioned by the country's political dictatorships, punctuated by periods of ineffective liberal democracy. Quite a few found

that by joining Freud with Karl Marx, they could better understand this oppressive history. By 1976, when the Proceso military government went after the Left, it too deployed psychoanalytic rhetoric in its defense of family and patriarchy. Psychoanalytic thought and culture had become so deeply central to Argentina that it could survive and even sometimes be superficially deployed by terrorist dictatorships.

This is the essential narrative line in Plotkin's book, which is prodigiously researched and clearly written, informs enormously on a great many particulars, and is a major work of synthesis on the evolution of psychoanalytic culture in Argentina. It also falls short on several counts. For one, the study describes but never ends up adequately explaining the centrality of psychoanalysis in Argentina. The construction of Plotkin's detailed chronological narrative gives us many "whats" but very few "whys." Psychoanalysis took hold in a big way and then markedly declined in many parts of the world, such as the United States. The issue, I suspect, is its tenacious capacity to survive and grow on Argentine soil. But why? At several points, Plotkin indicates that psychoanalysis applied simultaneously to traditional values like patriarchy but also to innovative social and political change. Yet he does not elaborate on this area of potential appeal. At other points, he suggests that psychoanalysts often tended to see themselves as public intellectuals and tried, often quite successfully, to make their case beyond limited professional audiences. But he does not develop this potentially fruitful point to account for widening psychoanalytic influence. Why did Argentine psychology open itself to psychoanalysis as it did not do, very markedly, in most other countries? Again and again, through diverse contexts, Plotkin notes how very profoundly psychoanalysis appealed to Argentine women, but his description tends to be unaccompanied by sustained explanation.

Another problem is the range of phenomena that Plotkin includes within Argentine psychoanalytic culture. For the most part, he refers to an enormous body of popularizations of portions of Freudian, Kleinian, and Lacanian thought. Most of these popularizations reference human interiority and feelings, drives, and fantasies. For Plotkin, this is sufficient to describe a proliferation of psychoanalysis. More accurately, he seems to be describing a protean, ever-changing, semi-psychoanalytic disposition. While this may help to explain the cultural diffusion of the phenomenon, one is often at a loss to say precisely what is being diffused. Aside from the pronouncements of members of the APA and other psychoanalytic bodies, the massive material Plotkin presents has little doctrinal consistency or commonality in clinical orientation. Obviously, there have been elements in Argentine culture thirsting to express inner feelings within a respectable intellectual doctrine. Plotkin needs to bring us closer to these, especially to pervasive feelings of rootlessness among large portions of the population with Jewish and European roots.

Finally, the Argentine psychoanalytic experience needs to be compared to that of other nations. In recent years, the most fruitful advances in the history of psychoanalysis have come by international comparisons. Given the obvious keen connection to Britain for Kleinian doctrine and France for Lacanianism, these are two good comparative areas. But APA founder Garma came from Spain, and cofounder Langer was from Austria—two other obvious comparative spots. The United States will also provide a fruitful comparative dimension, for by the 1950s psychoanalytic culture was as strong in the United States as it currently is in Argentina. We may better understand the nature and extent of the Argentine romance with psychoanalysis when we characterize it within a larger global map.

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JAVIER AUYERO. *Poor People's Politics: Peronist Survival Networks and the Legacy of Evita*. Durham, N.C.: Duke University Press. 2000. Pp. xiv, 257. Cloth \$54.95, paper \$18.95.

This book by Javier Auyero provides an in-depth sociological study of a particular shantytown in the suburbs of Buenos Aires, Argentina. Based on extensive field work and in large measure on oral interviews conducted in the mid-1990s, it aims to describe in intimate detail the daily lives of the inhabitants of Villa Paraíso (not the real name) and to tell, among other things, "about how politics intermingles with people's everyday lives" (p. 24). To this end, much of the book relates individual life stories to the larger political phenomenon of Peronism and the Peronist Party in Argentina, the movement created by and associated with Juan and Evita Perón. One of the main purposes of the book is to describe and analyze how the legacy of the Peróns continues to be important for many of the poor and dispossessed in Argentina and to investigate why this is so.

The first two chapters provide a grim and powerful picture of general conditions in the town. Extreme poverty, disease, and crime, especially acts of violence committed by members of the community against one another, are common features. Moreover, while the town has experienced change and improvement in certain areas such as street paving and the extension of utilities, much has remained the same in terms of the overall poor conditions and low standard of living. During the period Auyero was living in the neighborhood, the economic situation of most of its inhabitants deteriorated as unemployment increased substantially and the overall gap in the nation between rich and poor widened. While the author does not make the specific claim, it seems likely that these experiences in this particular site were common to similar poor neighborhoods throughout the country.

Chapter three describes the various networks established by political leaders to help the residents solve

problems and receive assistance. From top to bottom, this is a process controlled by Peronist loyalists. It ranges from the governor of the province and, notably, his wife, to the mayor, to various "brokers" who work on a one-to-one basis with individuals in need. While the story seems a familiar one of patron-client networks related to traditional political machines, Auyero seeks to underscore and explain some of the subtleties and complexities of how such relationships operate in this particular setting, arguing that the examples he observed were much more than a simple exchange of favors for votes and involved more complex emotional attachments and responses.

Chapter four provides a fascinating account of how female "brokers" consciously imitate the style, manner, and forms of expression of the legendary Evita Perón. At the regular campaign rallies that are held to distribute material favors as well as to retain political loyalties, these women engage in what the author calls "Eva Perón as a Public Performance," presenting themselves, as she did, as the "mother of the poor" of all Argentines and establishing "a bond of love" with the audience. Practices such as these, he suggests, help to explain the remarkable resilience of Peronism since the end of the first administration in a military coup in September 1955.

Chapter five focuses on how "clients" view "brokers." For the most part, Auyero argues, these views are favorable. While many in the town disdain what they call "politics," most have accepted the system of networks that dispenses favors, material goods, and a sense of caring. For the most part, brokers are viewed as sincerely committed to the interests of their clients and as effective in delivering whatever might be requested or needed. Moreover, it is this system on the local level that provides the key to Peronist resilience. While the "inner circles" of brokers are small in number, they "are crucial to the party's success, providing as they do not only the party's hard core of voters but also its political operatives. It is they who maintain the party structure between elections and keep support alive" (p. 180).

Chapter six discusses the "collective memory" of Peronism and elaborates on its resilience, especially in Villa Paraíso. Auyero relates Peronism's longevity to the ability of the original regime and subsequently the party apparatus to provide directly basic goods and services. In addition, Peronism has also been seen by the poor, in the 1990s as in the 1940s and 1950s, as extending to them a sense of self-worth and dignity that they had not had previously.

Overall, this is a thought-provoking and informative book. While there are a number of studies of shantytowns in the literature on Latin America, few focus so successfully and insightfully on the political networks that operate within them. As such, this is a book that will be of value to those interested in the general phenomenon as well as the particular Argentine variant. It adds, too, to our knowledge and understanding of the complex story of Peronism and its continuing

influence. For the general reader, the glimpse into the personal lives of the poor may be the most evocative and interesting aspect of this study. Less appealing are the rather complicated references to larger theoretical discussion and debates. Moreover, in the last analysis, the reader may not be convinced that the story Auyero tells of the clientelistic relationships in Villa Paraíso is as different from the traditional one as he argues. As he describes the process, it still seems to be a case of political leaders providing material incentives and favors, no matter how elaborately packaged, in return for votes. Be that as it may, there is no doubt that Auyero succeeds in effectively portraying the everyday lives of the inhabitants of Villa Paraíso and the various strategies they employ to survive the frequent ups and downs and uncertainties of the Argentine economy. In so doing, he offers an important glimpse into this particular shantytown and also suggests a more general picture of the lives of tens of millions of other people who live in similar conditions throughout Latin America.

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KARIN ALEJANDRA ROSEMBLATT. *Gendered Compromises: Political Culture and the State in Chile, 1920–1950*. Chapel Hill: University of North Carolina Press. 2000. Pp. xiv, 346. Cloth \$59.95, paper \$19.95.

Karin Alejandra Roseblatt has written a revisionist history of the Chilean popular front, a series of center-left electoral coalitions that held power from 1938 to 1952. In many ways, those administrations prefigured the multiparty governments that have ruled the country since the end of dictatorship in 1990. Earlier works on the popular front emphasized national political and economic developments, especially the promotion of democracy, industrialization, and public welfare. Roseblatt, however, concentrates on cultural and social changes in the working class, particularly among women. As a result, she opens up previously neglected topics—especially gender relations among subalterns—without ignoring their linkages to political parties and the state. By focusing on complex changes in the lower social strata, she paints a more positive picture of the accomplishments of this reform period than might be derived from macroeconomic data. Indeed, she uncovers significant enthusiasm for the popular front among the underprivileged. This is a solid and sophisticated book that should appeal not only to Chileanists but also to other students of social history.

Roseblatt's investigation stands on a sturdy foundation of original research, including numerous interviews, government documents, personal archives, pamphlets, and newspapers. She also bases her analysis on a broad array of secondary literature in Spanish as well as English and on theoretical and comparative studies as well as the Chilean case. She adds to the book some

graphic illustrations. The outcome is a very thorough and satisfying account.

Roseblatt makes three particularly innovative contributions. She is one of the few scholars to explore the evolution of the Chilean welfare state: its agencies, employees, policies, and impacts, particularly the reinforcement of the male-led nuclear family. The popular front's promotion of marriage placated conservatives as well as many progressives, although some women resisted their subordination. Roseblatt also explains how an unusual family wage system molded people's lives both at home and at work.

At the same time, Roseblatt provides pioneering dissections of discourses during the popular front era, thus unveiling cultural messages and controversies suffused with gendered language and notions. Among both leaders and followers, the meaning of this reform era evolved through dialogue and debate. The contested definitions of citizenship and politics recast the roles of the common people and the state.

And, above all, Roseblatt fills in the history of gender, women, and feminists. She shows that gender affected not only issues concerning family and sexuality but also questions about labor, capital, and national development. The author pays special attention to the key organization, Movement for the Emancipation of Chilean Women. Although these activists fell short of their loftier goals, their dedication and progress should not be underestimated. As Roseblatt reminds us, their often ambiguous victories and defeats were reflected in the reform movements that came thereafter.

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EUROPE: ANCIENT AND MEDIEVAL

MARY T. BOATWRIGHT. *Hadrian and the Cities of the Roman Empire*. Princeton: Princeton University Press. 2000. Pp. xviii, 243.

Any book on Hadrian's impact on the Roman Empire is welcome, and particularly one by Mary T. Boatwright, foremost in clarifying Hadrian's role in developing a truly Graeco-Roman world. Boatwright's incentive for this book was a desire to "illuminate the Roman empire of Hadrian's day" (p. 16), and, in general, she has succeeded well. Moreover, the book demonstrates that archaeology is essential for a full understanding of the historical past rather than being simply a supplementary source of information, as is often perceived.

The book has nine chapters, all fully referenced and written in a style that ably presents the nuances of their subject matter. The first evaluates the Roman Empire under Hadrian and his contribution to its magnificence; the second surveys the source material, verifying Hadrian's position as the emperor who established the greatest degree of cultural unity among the provinces. However, Boatwright wrongly asserts that the

second-century provinces did not generally suffer the “rapacious” officials who characterized later centuries (p. 9): the long list of transgressors during Trajan’s reign provided by Pliny the Younger is clear on this. Moreover, the author’s claim that Hadrian’s response to the Jewish Revolt of 132–135 “exemplifies Rome’s ruthless suppression of nonconformists” (p. 17) needs challenging. Boatwright’s perspective will be examined below, but Rome acted against “nonconformists” only when public order was directly threatened by their militant intransigence.

Chapters three and four detail the legal and administrative changes initiated by Hadrian in his agenda of civic renewal and development. Boatwright’s analysis of civic obligations (p. 44–56) is excellent, albeit mainly based on evidence from the western Roman Empire. The lack of detailed comment on the arrangements in the east is surprising, as much of her archaeological evidence comes from there. That apart, while one can agree with the author’s assertion that most *curatores rei publicae* were often “local” men with short-term commissions (p. 75–76), their appointment does indicate a degree of central oversight in the communities concerned and a need for management of their affairs.

The next four chapters examine the literary, epigraphical, and architectural evidence for Hadrian’s civic benefactions. Chapters five and six deal with these in a comprehensive way, and Boatwright reveals an enviable command of the relevant documentary material. She is mistaken, however, to declare that Petra gained municipal status under Hadrian (p. 104–105): Trajan was responsible. Otherwise, these chapters are only marred by a focus on Italy and the eastern provinces, and a general neglect of purely archaeological evidence. For example, there is no discussion of Hadrian’s renewal of the civic infrastructure of Britannia, amply proved by excavation if lacking contemporary documentation.

Chapter seven is an elegant appraisal of the archaeological and historical sources for Hadrianic activities at Athens, Smyrna, and Italica and the emperor’s personal impact on these three centers. Here Boatwright succeeds best at “illuminating” the Roman Empire at the time. As for chapter eight, this surveys Hadrian’s donations to Cyrenaica and Mysia and his foundations at Antinoopolis and Aelia Capitolina (Jerusalem). The evidence for these sites is carefully analyzed, but Boatwright’s summary of the events behind the rebuilding of Cyrene (the Jewish Revolt of 115–116) and surrounding the foundation of Aelia (the Jewish Revolt of 132–135) seems onesided. Having already denounced Hadrian’s anti-Semitism (pp. 17, 173), Boatwright ignores the role of the Cyrene Jews in the wanton destruction of their community and assigns responsibility for the 132–135 Revolt to Hadrian’s founding of Aelia and his banning of circumcision, a “deliberate attempt to destroy Jewish ethnic character” (p. 196–97). A wealth of historical evidence indicates that Rome went to great lengths to accom-

modate Jewish mores and practices, to integrate Jews into civic and political life: note the involvement of the Jewish community in Hadrian’s building program at Smyrna, for example. It was the incendiary activities of extreme fundamentalists that led directly to Hadrian’s “ruthless” suppression of the Palestinian Jews—and them alone.

Spelling errors are few (e.g. “principle” for “principal” on p. 123), terminology is occasionally inexact (e.g. *pax Augusta* for *pax Romana*), but Boatwright has not been well served by her editors. Epigraphic references, for one, are inconsistently cited, some as numerals (e.g. *CIL* VI 967), others with a preceding # symbol (e.g. Smallwood, #476): of the two styles, the first is usual and less unpleasantly conspicuous to the eye. More worrying is the inadequate index. Oinoanda, to take one example, is mentioned in the text on no less than nine pages, but is indexed under “Demostheneia,” a musical event exclusive to that place.

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ELIZABETH DePALMA DIGESER. *The Making of a Christian Empire: Lactantius and Rome*. Ithaca: Cornell University Press. 2000. Pp. xv, 199. \$39.95.

In the days when every history of Christian thought was a history of dogmatics, there was only space for Lactantius in the margins; so long as it seemed unthinkable that politics and piety could be lawfully united, the Christian Cicero was not judged worthy of a monograph in English more substantial than R. M. Ogilvie’s *The Library of Lactantius* (1978), which is admittedly a classic of its kind. Students of Neoplatonism continue to ignore this Latin adversary of Porphyry, while historians are gratified to imagine that he was innocent of the controversial prejudices that cast persistent doubt on the veracity of his episcopal contemporary, Eusebius. To write a book like the present one, an author must be willing to believe that ideas make history—perhaps, indeed, that nothing changes history so much as an idea.

First, one must suppose that the pagan emperors Diocletian and Maximian desired to be taken seriously when they took the names of Jove and Hercules as personal epithets; Elizabeth DePalma Digeser’s introduction explains, with a rare combination of accuracy and clarity, how the troubles of the third century made such pretensions seem not only tolerable but necessary, and thus prepares the reader for the argument in chapter one that the mockery of the archetypal Hercules and Jove in the *Divine Institutes* is not the shadow-play that it would have been in other periods but a spirited satire on two living gods. Chapter two is another in which a promising hypothesis is supported by a pertinacious scrutiny of the evidence, the aim being to show that when Lactantius endorses a theory of natural law in the *Divine Institutes*, he is tacitly contrasting the views of Ulpian and Cicero with the

tyranny of a polytheistic Dominate which refused to see that universal monarchy wielding universal law must find its sanction in the worship of a universal God.

Platonists and Christians often took the same side in intellectual warfare under the empire, but as chapter three reminds us, they stood consciously on different sides of the law. It is Lactantius himself who urges—more consistently than previous apologists, Greek or Latin—that the Platonists and Hermeticists should make common cause with the church because they held a common notion of the deity. His case, in my view, is more tendentious than Digeser acknowledges when it comes to religious practice, for though she speaks of Platonists as worshipping the first principle, her notes do not reveal that they offered anything to the one that we should normally distinguish by that term. Nor is it clear to me why she infers that the discovery of Coptic renderings from Hermetic treatises should add any weight to the theory that they contain material of Egyptian provenance (p. 66): Plato's *Republic*, after all, is one of the texts that is partially translated among the codices from Nag Hammadi. Her arguments do persuade me, however, that Porphyry was most probably the philosopher who conspired with Hierocles to warm the fires of Diocletian's persecution. The opinions of John J. O'Meara, Pier Franco Beatrice, Timothy D. Barnes, and others on the structure and date of the treatise against the Christians are reviewed with fluent mastery, though I fear that the less industrious reader will overlook the long endnote to which the publishers have banished the discussion (pp. 161–63). A more extensive bibliography might have been consulted for the dating of Arnobius's apology and the *Church History* of Eusebius, for it seems to me that Brian Croke and Michael Bland Simmons have yet to prove that either of these works antedated the first edition of the *Divine Institutes* (pp. 10, 96).

All chronologies grant that it was possible for Lactantius to leave his mark on the Emperor Constantine, and many scholars have noticed anecdotal similarities between his thought and that of the *Oration to the Saints*. As Digeser demonstrates in chapter five, however, the two men were united more profoundly by an uncommon religious policy: espousing forbearance rather than persecution, both chose that form of it, here defined as concord, which does not merely permit the coexistence of truth and error but anticipates the peaceable conversion of the subjects to the religion of the sovereign. Historians have all too often assumed that a Christian ruler could have only sinister or pragmatic reasons for condoning the survival of paganism; some contend that as Constantine did not suppress the cults he was insincere, while others retort that as he was sincere, he must have suppressed them after all. Digeser, with better insight, takes the view that even a Christian may sincerely decline to force a new religion on his subjects: the detail of the controversy is left to another long endnote (pp. 167–71),

which overlooks the evidence of Optatus, *Against the Donatists* II.17. But so do even the best of her opponents, and in any case this partisan testimony cannot overrule the silence of the *Theodosian Code*. By urging, against Eusebius and Barnes, that Constantine was as patient as he claimed to be, and by arguing, against Jacob Burckhardt, that Lactantius had already framed a doctrine that would justify such indulgence, Digeser has done justice to two great figures who were respectively the architect and the advocate of Christian liberalism.

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CLAUDE CAROZZI. *Apocalypse et salut dans le christianisme ancien et médiéval*. (Collection historique.) Paris: Aubier. 1999. Pp. 230. FR 130.00.

In pursuing his analysis of eschatological beliefs during Christianity's first sixteen centuries, Claude Carozzi has produced a sensitive and insightful, if not comprehensive, account of evolving modes of ideological thought in the elaboration and in the culture of an apocalyptic mentality. Carozzi holds that ideology alone, which he defines as a cognitive posture between empiricism and theology, enabled Christians to fashion their expectations concerning the anticipated end of time since they could not comprehend eschatology from either empirical observations or from the bewildering symbolism of neotestamentary texts. Carozzi's argument is anchored by the text of the highly influential *Libellus de Antichristo*, written in 953–954 by the monk Adso of Montier-en-Der (Lorraine), which is here presented as the linchpin of apocalyptic constructs. The *Libellus*, first, incorporates major if contradictory trends of earlier apocalyptic exegesis. Second, the *Libellus* arrays these earlier ideas along an exegetical axis that was at once imperialist, dualistic, and prophetic but that imputed no eschatological role to the church. Rather, it foresaw an ultimate (Carolingian) emperor who would first unite Christendom and rule in peace for years before later abdicating in Jerusalem, thereby ushering in the reign of an Antichrist and the final struggle between his diabolical church and the small group of elects to be led by the prophets Eli and Enoch. Third, although incoherent and only loosely based on scripture, this treatise came to govern people's behavior because it offered a myth whose ideology linked the apocalypse to daily life, allowing the perception of human events and actions as signs and modulators of the end to come. Fourth, Adso's scenario was atemporal enough to support ongoing attempts at actualization well into the sixteenth century.

Prior to the *Libellus*, the established church had opposed the millennial element of eschatological exegesis that promised the coming kingdom of heaven on earth. After the *Libellus* provided an enabling rhetoric, eleventh and twelfth-century papal reformers formulated an apocalyptic scenario that presented their goals

in messianic terms and attacked their enemies as Antichrist, only to be considered agents of Antichrist by Joachim of Fiore (d. 1202) and his followers. From then on, eschatology became irreconcilably polarized with the competing visions of apocalypse and penance vying for superior efficacy as roads to salvation. Apocalyptic millennial beliefs, where salvation was the exclusive privilege of the predestined elects identified with those who would be led by Eli and Enoch at the end of time, flourished within dissenting milieux headed by, among others, John Wyclif (1330–1384), Jan Hus (1369–1415), and Thomas Müntzer (ca. 1490–1525). The church's salvific system, by contrast, was situated in the present and revolved around the individual, resting on a complex penitential discipline that offered multiple means for the attainment of salvation: confession, indulgences, jubilees, and purgatory.

Underlying the tensions surrounding eschatological exegesis throughout the Middle Ages was a battle for the control and for the definition of time. Millennialism, Carozzi argues, insisted upon the renewal rather than upon an end of time and was animated by a nostalgia of origins that prompted its adepts to seek a return to apostolic Christianity if not to prelapsarian paradise. Thus, Raoul Glaber, often invoked as a living witness of the apocalyptic fears of the year 1000, becomes in Carozzi's interpretation the chronicler of a jubilee celebrating the renewal of the world. The revival of apostolic values, such as poverty, which informed newer monastic as well as heretical experiments, is interpreted by Carozzi as the manifestation of an eschatology understood to reflect an inversion of the myth of origin contained in the Gospels. Similarly, Carozzi stresses that Joachim of Fiore, traditionally considered the revitalizer of medieval millennialism, was in fact operating within a circular concept of time in predicting the end (in 1260) not of "the world," but of "a world."

Penitential salvation, on the other hand, had articulated its earliest formulations and methods in opposition to the great cycles of renewal staged in the late Roman Empire. It had thus favored a conception of time based on an ongoing linear sequence of discrete liturgical years unencumbered by any cyclical structure that might group them into metaphysically significant units. In the penitential scheme, the end of time was achieved not through a progressive perfection of the world but by the irresistible expansion of an immutable truth given once and for all. Progress was conceivable only at the level of individual salvation, the result of an intersection between personal contrition and confession and that universal solidarity presumed to unify all faithful through the ages. Since the means of salvation were available in this world, through penance and intercession, the apocalyptic scenario was rendered timeless, not to say meaningless.

Penitential salvation, as it was strongly elaborated by the church from the thirteenth century onward, was inscribed both in the long duration and in terrestrial time. Apocalyptic salvation, by contrast, operated in

the short term and within a cyclical time frame of renewal. Thus, a principal implication of Carozzi's conclusions is that eschatological scenarios were less about the end of time than about time itself and the means and meaning of salvation. Although persuasively argued throughout, this essay might nevertheless have profited from a deeper engagement with the historiography it so elegantly challenges.

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NATALIA LOZOVSKY. *"The Earth Is Our Book": Geographical Knowledge in the Latin West ca. 400–1000.* (Recentiores: Later Latin Texts and Contexts.) Ann Arbor: University of Michigan Press. 2000. Pp. viii, 182. \$44.50.

During the nineteenth century, when geography was in the process of becoming a university discipline, the faculties in this new academic specialty sought to elaborate the history of their field. In the resulting tradition of the history of geography, the early medieval period, ignorantly characterized as the "Dark Ages," is represented as the nadir of geographical knowledge in the West. The focus of these histories of geography are technical works in the genre of Dicu's *Liber de mensura orbis terrae*, which was written at the court of Louis the Pious ca. 825. These books are condemned for a lack of empirical research and for copying earlier Roman materials. In short, they did not advance geographical knowledge.

Natalia Lozovsky, in this revision of her dissertation, does not take issue with the highly negative characterization of early medieval geographical knowledge. Rather, she is interested in "the early medieval view of how geographical tradition fit into the system of knowledge of the time" (p. 6). She argues that men such as Dicu were writing for philosophical and theological purposes and that neither concerted empirical research for the purpose of finding new knowledge nor significant deviation from Roman models was part of their program. In short, Lozovsky believes that early medieval geographers were not really interested in geographical reality.

Lozovsky is correct in making clear that the men whom she has studied, both geographers and historians, had both philosophical and theological interests that can be identified in their work. However, she is at fault for accepting the state of the question provided by those who have written the history of geography, who condemn early medieval geographers for their failure to provide useful and accurate geographical information to their readers. Rather, an understanding of the material world in which these geographers lived makes it clear that they had sound practical reasons for presenting, although sometimes in modified form, the information that they found in later Roman geographical works. In short, men such as Dicu had deep

philosophical and theological interests while at the same time serving as practical geographers.

It is manifest that the physical geography of early medieval Europe did not differ significantly from what it had been during the later Roman Empire. For example, mountains, rivers, and coastlines did not disappear or suddenly appear in the five centuries between Constantine the Great and Charlemagne. In addition, the administrative geography of early medieval Europe also was quite similar to that of the later empire. The Roman road system survived quite nicely throughout the Middle Ages. The great fortress cities (*urbes*) remained the administrative and political centers of power and population concentration. The *civitates* remained the political administrative units under comital jurisdiction. In addition, the Latin names of cities, towns, and even lesser places remained largely unchanged. Where they were altered, the early medieval geographers generally provided appropriate glosses. In short, early medieval geographers cannot be condemned for presenting late antique geographical knowledge regarding roads, cities, towns and no less importantly the distances or travel times between them. Like much else in the early medieval world, the physical and administrative geography of the later Roman Empire endured.

No less important to our understanding of geographical knowledge in the Latin West are a plethora of relevant text genres neglected by Lozovsky. The vast tradition of *agrimensores* with maps and diagrams, and the pilgrim guides, to name but two of the most obvious, demonstrate a first-rate knowledge of geography in the early Middle Ages. These texts, incidentally, also have been neglected by geographers writing the history of their field. In addition, the verbal maps that fill early medieval charters, undoubtedly based upon the field work of the *agrimensores*, are as accurate and detailed as any property descriptions left by the Romans. Finally, the vast amount of geographical knowledge available to early medieval governments for the deployment of their soldiers is remarkable. Charlemagne, for example, often deployed three or four widely separated armies 500 kilometers or more distant from a particular area of would-be concentration in highly sophisticated pincer movements that worked with clockwork precision.

We are indebted to Lozovsky for identifying various philosophical and theological views found in the works of early medieval geographers. In addition, her book is filled with much interesting detail and has an excellent bibliography. Much more research is needed, however, before scholars gain a firm grasp of geographical knowledge in early medieval Europe and dispel the myth of the "Dark Ages."

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RACHEL L. STOCKING. *Bishops, Councils, and Consensus in the Visigothic Kingdom, 589–633*. (History, Lan-

guages, and Cultures of the Spanish and Portuguese Worlds.) Ann Arbor: University of Michigan Press. 2000. Pp. viii, 217. \$42.50.

Visigothic Spain has not received the scholarly attention among early medievalists accorded some other regions of Europe, especially by anglophone historians. The strong liturgical and legal traditions of the kingdom are offset by a comparative paucity of narrative documents addressing social and cultural questions about Visigothic society. Consequently, Rachel L. Stocking's book on bishops and councils in the Visigothic era is welcome, especially as it combines a close reading of the conciliar sources of the sixth and seventh centuries with an investigation into the social and cultural issues from which they arose.

Stocking's discussion of Visigothic conciliar history is framed rather tenuously around the issue of consensus. In reflecting upon what she views as "a fundamental paradox in the exercise of social power," Stocking notes that in the absence of a centralized coercive authority, judicial decisions depended on a "community's shared recognition" of a tribunal's authority, an authority that relied on "consensus." Conciliar authority depended on episcopal harmony and a "practical functioning consensus among all the communities of the kingdom." Unsurprisingly, Visigothic sources reveal many arenas of disagreement and disunity. Stocking cites this disagreement to argue that the history of conciliar consensus in Spain was "fragile," "coercive," and fraught with suspicion. As presented by Stocking, the stated paradox in social power is at least questionable, and in places the argument approaches tautology.

Stocking reaches terra firma once the Visigothic conciliar legislation is directly engaged. Although set in the wider context of Iberian conciliar history, Stocking's investigation focuses on councils between 589 (Toledo III) and 633 (Toledo IV). Toledo III was convened by King Reccared, recently converted to Catholicism from Arianism and intent on inaugurating a vision of a Catholic kingdom and resolving the practical problems that arose from that change. Toledo IV, dominated by the political philosophy of Isidore of Seville, is often viewed as a mature statement of political and religious cooperation in the kingdom. Stocking argues that these councils failed to uphold consensus because the reality of Visigothic political life did not live up to the ideals enshrined in the optimistic language of their prefaces, cautioning that "it is especially problematic to assume any direct relationship between those tradition-bound assertions of ideal consensus and specific local mechanisms of practical consensus." Royal interests competed with episcopal interests, those of Catholic bishops with Arian bishops, and those of bishops with secular authorities in the provinces. Some tensions are more extensively documented by Stocking than others. For example, there is little evidence to illustrate or substantiate the oft-mentioned tension between collective and individual episcopal authority, and Stocking must

rely on speculation about what those divided interests must have been, rather than demonstrating what they actually were. Where such evidence is abundant, however, Stocking shines. Her chapter on the provincial councils held between Toledo III and IV illuminates the atmosphere of suspicion that dogged Catholic-Arian relations in the years after the royal conversion, and there is much of interest in her contextualization of the development of a highly ritualized conciliar procedure in Spain.

Regrettably, Stocking fails to include any substantial overview of the current state of research on Spanish councils. Indeed, Spanish-language scholarship overall is less well represented than one would have liked and often appears to have been sacrificed in favor of scholarly literature on Merovingian topics, a decision that is not satisfactorily explained. (Indeed, it could be argued that Merovingian appeals to consensus were played out in an ecclesiastical tradition that differed from the Romano-Visigothic in precisely those areas that are the subject of this book: conciliar practice, regional diversity, educational and liturgical reform.)

In sum, despite its shaky start, there is much to admire in this book. Stocking's analysis of specific councils and the contentious bedrock from which they arose is vigorous, scholarly, and engaging, especially when narrative sources supplement conciliar analysis. The attempt at a broad conceptual framework, even if not always successful, enables this book to transcend some of the boundaries that often hem regional analyses of early medieval sources. In highlighting the immense diversity of social, cultural, and religious concerns that underlay expressions of authority, Stocking's book has much to engage the early medievalist.

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JOSEPH PÉREZ and SANTIAGO AGUADÉ NIETO, editors. *Les origines de la féodalité: Hommage à Claudio Sánchez Albornoz*. (Collection de la Casa de Velázquez, number 69.) Madrid: Casa de Velázquez. 2000. Pp. 253. 160.00 FR.

The conference at which the contents of this volume were first presented took place in 1993 at the University of Bordeaux, where Claudio Sánchez-Albornoz, Spain's greatest twentieth-century medieval historian, spent the years from 1936, when he resigned his post as the Spanish ambassador in Lisbon, to 1940, when he departed for Argentina. It was at Bordeaux that he wrote the bulk of his great work, *En torno a los orígenes del feudalismo* (3 vols., 1942). In this and other studies, he developed theses that held the field in Spanish historiography for half a century: Spanish exceptionalism; the immature or incomplete nature of Spanish feudalism, despite the prefeudal nature of Visigothic society; the influence of the frontier on Spanish history; the centrality of the Castilian experience; the importance of a free peasantry; and many others. Sánchez-Albornoz lived long enough to see many of

these positions overturned, so while vehemently disagreeing with the findings of some of the papers in this volume, he would not have been surprised by them.

Only one paper confronts directly one of Sánchez-Albornoz's theses: José María Minguez again attacks the idea of the total depopulation of the Duero valley during the earliest stages of the *Reconquista*. Others address topics on which Sánchez-Albornoz wrote, without focusing on his ideas. Santiago Aguadé Nieto, in a rambling study (composed almost entirely of one and two-sentence "paragraphs") that takes up nearly a third of the volume, considers the chronology of feudalization and its relationship to urban development in Asturias, the region to which Sánchez-Albornoz devoted his earliest investigations. In an interesting exercise in comparative history, Carlos Estepa juxtaposes the Spanish *hombres de behetría* (the subject of groundbreaking articles by Sánchez-Albornoz from 1924 and 1927) and *labradores del rey* with the German *Königsfreie*, all as examples of primitive royal-seigneurial jurisdiction. Eric Bournazel and Jean-Pierre Poly transfer the concerns of Sánchez-Albornoz to non-Iberian contexts. The former offers brief reflections on feudal kingship under the Capetians from both institutional and ideological perspectives. The latter tackles the question of the survival of Roman institutions in a provocative essay on the Salic law, the core of which, he argues, derives from administrative regulations for non-Roman frontier troops dating from the fourth century. José Luis Martín offers homage in a different way, glossing parts of a speech on agrarian reform given by Sánchez-Albornoz in his capacity as a member of the Cortes in 1932. Pierre Bonnassie barely mentions Sánchez-Albornoz in his examination of feudal institutions in the *Vielles Coutumes de La Réole*, a rare inventory of seigneurial rights dating from the eleventh and twelfth centuries.

These authors avoid Sánchez-Albornoz's theses not out of piety, but because the concerns of scholarship on feudalism have moved on, a fact noted by Julio Valdeón Barúque in his discussion of the place of Sánchez-Albornoz in the historiography of Spanish feudalism. Of greater interest to most of these scholars in 1993 was the debate over *la mutation de l'an mil*. Guy Bois's controversial book of that title appeared in 1989; Dominique Barthélemy threw fat on the fire with his review of Jean-Pierre Poly and Eric Bournazel, *La mutation féodale: X^e-XII^e siècle* (2d ed., 1991), which appeared in *Annales* in 1992. Barthélemy's essay in this volume, a careful critique of the work of Georges Duby and Bonnassie, is one of the better explanations of his position in print; the editors include a response by Bonnassie and a last word from Barthélemy. Bournazel (p. 130) and Poly (p. 196) hint at the debate in their own essays in this volume, and even the Spanish scholars are drawn in. Josep Maria Salrach's detailed description of the evidence for disputes involving the Catalan monastery of Sant Cugat del Vallès begins with attacks on Barthélemy's position, while Aguadé draws on Bois and Bonnassie. While most of the

authors make at least some reference to the putative subject of the conference, *mutationnisme* ends up overshadowing Sánchez-Albornoz.

As both a volume of conference proceedings and a Festschrift, this collection of essays suffers from the faults common to both of those genres: papers of widely divergent length, quality, and intent, as well as only loose adherence to a common theme. It labors under the further burden of a seven-year delay between conference and publication, as much of the work presented here will already be familiar to specialists. In this case, however, the delay has a positive effect, too: perhaps the most interesting aspect of the collection is its recording of a particularly heated moment in recent debates about feudalism.

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BERNARD S. BACHRACH. *Early Carolingian Warfare: Prelude to Empire*. (The Middle Ages Series.) Philadelphia: University of Pennsylvania Press. 2001. Pp. xii, 430. \$55.00.

This study, the work of a mature scholar, is the most important (and controversial) book on Carolingian history since the debates between Henri Pirenne and Alfons Dopsch came to an end more than a half century ago. Bernard S. Bachrach states his thesis clearly. Pippin II (d. 714), an early Carolingian major domo, conceived a "grand strategy" designed to piece the fragmented Frankish kingdom of the Merovingians back together. Although Pippin did not live to see this project through to its completion, his son Charles Martel and grandson Pippin III successfully persisted in his policies. When Pippin III died in 768, the Frankish kingdom was intact, and the dynasty had risen above the office of major domo to assume the royal title, "kings of the Franks."

In the pursuit of their ambitious goal, the early Carolingians developed a sophisticated military organization that allowed them to put large armies in the field year after year and to support them logistically. Carolingian armies consisted of expeditionary forces that they called up from a militarized civilian population of middling property owners who possessed sufficient wealth to equip themselves for campaigns beyond the confines of their local districts. Large landholders, who retained military households commensurate with their wealth, led or sent to the hosts professional soldiers who were heavily armed and highly trained to fight both on horseback and on foot. Officials (bishops, abbots, and counts) contributed armed men on the basis of the wealth of the institutions that they headed. The most important source of well-equipped forces, however, came from the royal following, the obsequium. Some of these men were continually at the court; others were assigned to military colonies and royal palaces scattered throughout the kingdom. Local defense forces were available, but largely unnecessary, since the early Carolingians engaged primarily in

offensive operations. Bachrach estimates that by the end of Pippin III's reign, Frankish rulers had a manpower pool of approximately two million men who were able to bear arms in some capacity—although nowhere near that number was ever summoned to active service during a campaigning season.

Large armies were essential because the Carolingian grand strategy rested on the principle of "overwhelming force." Behind the ramparts of impressive fortifications, other powerful leaders within the Frankish kingdom ensconced themselves, hoping to maintain their de facto autonomy. To overwhelm these strongholds required offensive forces that were at least three times larger than those under siege. In addition, those conducting the siege had to be protected and supported by men not directly involved in the investment. For example, among the primary roles for the highly touted Carolingian cavalry were the protection of the besiegers from relieving forces and the scouring of the countryside for additional resources. Although cavalry was useful in warfare, it was not the decisive tactical element, as so many scholars have maintained.

While the early Carolingians practiced the doctrine of overwhelming force, they did not squander manpower or other resources. Each campaign was well planned and had specific, limited goals, which were achieved through diplomacy whenever possible. Their grand strategy, it is important to note, was to restore the Frankish kingdom, not to create an empire. While Pippin III did intervene twice in Italy, he had no ambitions to rule the Lombard kingdom. Nevertheless, this early Carolingian military organization became the vehicle that Charlemagne used to reestablish the Roman Empire in the West, hence, the book's subtitle. Hopefully, Bachrach will write a sequel to the present work dealing with the transformation of strategic doctrine under Charlemagne.

The evidence supporting Bachrach's conclusions is every bit as overwhelming as Carolingian forces were. Large Frankish armies, adequately trained, equipped, and fed, were led by men who had well-conceived ends and who had been educated in the art of war. This thesis will not be well received by some medievalists, who cling to notions that Carolingian military organization was primitive and that armies consisted of no more than a few thousand men who fought as individuals, not as units, much less as a tactically flexible whole having the capacity of adapting to a range of contingencies. According to these scholars, virtually no Carolingian military organization existed because they believe that there was no Carolingian state to support it. There was no policy-making apparatus; hence, there could be no grand strategy. Nor were there institutions staffed by officials who could implement policy. The Carolingian empire rose, according to this paradigm, because a coterie of Frankish aristocrats from the Rheinland agreed among themselves to use their minuscule military followings to pillage their neighbors' territories. They were successful for a while, but, when the opportunities for easy booty disappeared, these

warlords began fighting among themselves for the remaining crumbs, and this stateless society, based entirely on plunder, collapsed. This interpretation of Carolingian history, sustained by assumptions that persist only because many medievalists simply take them for granted, cannot be supported by the sources or by logic. More than thirty years ago, Karl Ferdinand Werner refuted the hypothesis of small early medieval armies. Yet, scholars continue to write that Charlemagne could raise an army of no more than five thousand men, while, at the same time, citing Werner as their proof. Obviously they have never read Werner. I hope that Bachrach's lengthy tome, which cannot be ignored, will force them to reconsider their views.

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W. SCOTT JESSEE. *Robert the Burgundian and the Counts of Anjou, ca. 1025–1098*. Washington, D.C.: Catholic University of America Press. 2000. Pp. x, 206. \$54.95.

Scott Jessee's book is a biographical study of a prominent member of the upper nobility in the western French province of Anjou in the later eleventh century. A sufficient quantity of charters ("over a hundred"; p. 8) has survived to enable the author to reconstruct in broad outline the long political career of one of the foremost Angevin castellans and evaluate his role in the conflicts pitting the counts of Anjou against neighboring enemies in Maine, Brittany, and Normandy.

Exceptional in not being a native Angevin, Robert was Burgundian in origin (hence his surname) and a direct descendant of both French royalty (King Robert 996–1031) and the counts of Never. As a younger son, he had no opportunities to inherit either title or offices in Burgundy; thus he came, or was sent, as a child to seek his fortune with Count Geoffrey Martel in Anjou, who had married Robert's great aunt, Agnes of Poitou. During the 1050s, the Angevin count rewarded Robert for faithful service with the command of two castles controlling frontiers with Brittany (Craon) and Maine (Sablé) in the northwest. These two strongholds, supplemented by later acquisitions, formed the nucleus of what became one of the most powerful territorial lordships in northern Anjou and made Robert a key figure in Angevin campaigns to resist Norman incursions under William the Conqueror in the 1070s and 1080s.

Jessee devotes most of his book to clarifying the role of his subject both in the internal conflicts of the later eleventh century (i.e. the struggle between Fulk Rechin and Geoffrey the Bearded, two descendants of Geoffrey Martel, for the comital office in Anjou in the 1060s) and the wars with Maine and Normandy. He also examines Robert's relationship with regional bishops and monasteries and ends with the latter joining the movement of the First Crusade. In his evaluation of life and accomplishments, Jessee finds that Robert

the Burgundian was outstandingly successful in advancing and strengthening the position of his family among the nobility of Anjou, but he also insists on seeing this castellan as a dedicated supporter of the larger interests of the counts of Anjou against the Normans and the Manceaux.

This monograph is an example of the most recent stage in scholarly investigation of medieval aristocracies. In *La Société féodale* (1939), Marc Bloch laid the groundwork when he argued that the institutions and mentality of feudal society—that is, of the ruling aristocracy—underlay much of the Western European world of the eleventh through the thirteenth centuries (he ranged geographically from England to northern Italy) and were essential to understanding it. In the decades after World War II, Bloch's famous book inspired many monographic studies that sought to better understand that world through detailed studies of the aristocracies of limited regions—counties, duchies, principalities—with Georges Duby's work on the Maconnais being perhaps the most influential. In studies like Jessee's this trend comes to its ultimate conclusion with efforts to penetrate into the realm of the medieval nobility through biographies of prominent and representative noble families and individuals. Jessee's nuanced portrayal persuades me of the value of such an undertaking, although I am not convinced that he has demonstrated that Robert the Burgundian was a "careful and rational man who worked patiently at being a diplomat, judge, political figure, even a reformer of the church" (p. 171).

This book features the normal scholarly apparatus of a bibliography, map (rudimentary), genealogies, and index. A register of Robert's charters, as well as a list of other sources mentioning him would have been a service to future historians seeking to write similar studies of other Angevin nobles of the period. This information can now be gleaned only by filtering through the footnotes.

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ADAM J. KOSTO. *Making Agreements in Medieval Catalonia: Power, Order, and the Written Word, 1000–1200*. (Cambridge Studies in Medieval Life and Thought, number 51.) New York: Cambridge University Press. 2001. Pp. xix, 366. \$64.95.

The last two decades have witnessed a good deal of controversy regarding document classification within the categories of public and private materials. We have also seen considerable debate concerning the nature of feudal theory and practice. Adam J. Kosto's work touches on both of these areas by analyzing Catalan documents called *convenientiae*. These documents, establishing all manner of landholding agreements between individuals, first appeared in the tenth century in Italy, France, and Spain. Heretofore the Catalan materials have not been extensively studied. The emergence of the county of Barcelona and its acquisition of

the neighboring territories that came to comprise the medieval state of Catalonia are here traced at a very basic level by examining a large body of *convenientiae*. While this category of documents has been discussed in Italy and southern France, Kosto offers the first systematic examination of this genre in northeastern Iberia. The region lends itself to such an examination with its remarkably rich archives and its evolution of a particularly complex governmental structure.

Beginning with the first surviving document from around 1020, Kosto elaborates the forms of their early use in a variety of situations. One of the earliest lines of expansion consists of castle tenures recorded in these documents, indicating chains of command that established the responsibilities of the chief tenant and his subordinates. *Convenientiae* relating to treaty relationships among the counts of the region also appear under this form, as do dispute settlements, family settlements, and promises. But the format was not exclusively aristocratic, as agrarian contracts between nobles and the peasantry also existed. Kosto goes beyond the categorization of document types to probe the nature of their enforcement over time. He then examines the sanctions provided by penalty clauses and sureties, the latter taking the form of property, personal duties of the grantors, and the use of hostages. These documents most commonly allowed grace periods of thirty to sixty days before the application of the sanctions. Initially a second *convenientia* sometimes contained an oath to reinforce the original document, but by the twelfth century the oaths were incorporated into the context of the original document. Kosto also offers interesting theories regarding the purpose of the addition of vernacular words into the Latin of the documents, speculating that they were designed for the reading of the documents to an audience, accompanied by appropriate gestures to bind the contractual parties more firmly.

During the last half of the eleventh century, the number of *convenientiae* grew geometrically as more individuals of all classes, but especially the counts, used them to formalize agreements. These documents were less formal in their construction than other contemporary parchments, due to the newness of their concept. The author argues that the changes in their documentary language during this period seem largely to have been determined by the court of the count of Barcelona, a sign of an increasingly active chancery and the growing power of that figure. He sees this as a sign that the counts and not the lesser feudality made the critical innovations in these agreements, in contrast to some feudal theorists who make the case for a bottom-up inspiration. The contributions of the episcopal chanceries to *convenientiae* were largely focused on acquiring titles to frontier castles, although in this case the nature of the legal control is not made clear in the context. These documents, with their more forward-looking procedures, replaced the older system of comital *placitum* by the end of the eleventh century. The twelfth century witnessed the renewal of older

convenientiae by new parties, as well as the spread of their use to other scribes and chanceries. As the century progressed, a gradual alteration appears in the format, especially during the reign of Alfons I, count of Barcelona and king of Aragon. The generally egalitarian nature of the eleventh-century *convenientiae* evolved into a relationship of superior distance from inferiors, as fief replaced commendation in the establishment of castle tenures. Kosto closes his study with an examination of the process by which the written document became an instrument of power management by the recorded extension of obligations extending over greater time periods: "the use of writing to construct the future" (p. 294). His study merits careful consideration by institutional historians.

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ALFRED HAVERKAMP, editor. *Hildegard von Bingen in ihrem historischen Umfeld: Internationaler wissenschaftlicher Kongreß zum 900 jährigen Jubiläum, 13.-19. September 1998, Bingen am Rhein*. Mainz: Philipp von Zabern. 2000. pp. 637. DM 99.00.

In 1998, the nine-hundred-year anniversary of the birth of Hildegard of Bingen was celebrated by scholarly meetings that resulted in a number of volumes devoted to her works. Alfred Haverkamp's collection of nineteen substantial essays surely ranks among the best. Divided into five uneven parts (urban context; the Benedictine framework; spheres of inspiration and influence; natural science; reception), it opens up many new perspectives, in particular for the reception of Hildegard's extraordinary output and its manuscript context. Four of the essays are written in English, the rest in German.

Haverkamp opens the volume with a study of the contrasts between the Disibodenberg, Hildegard's home for close to forty years, and the Rupertsberg in Bingen, locus of her own monastic foundation, as they are apparent in Hildegard's mostly imaginary *vitae* of St. Disibod and St. Rupert (ascetic recluse vs. urban sophisticate). Haverkamp shows convincingly that Hildegard's move to Bingen's urban environment (i.e. from the periphery to the center) signaled a conscious decision to turn her back on the life of recluse (and the ascetic ideals her first abode stood for) in favor of a public role of preaching, travelling, and oral communication that, finally, proved more important than her writings. At the same time, as Rudolf Holbach shows, her letters to the archbishops of Mainz, Trier, and Cologne place Hildegard within a web of societal and parental relations, although her political stance remained always vague, even in letters to such important prelates. Was Hildegard consulted by Jewish groups in her new urban environment? Angela Carlevaris doubts that theological concerns were ever addressed in Hildegard's actual dealings with the Jews. The views expressed in her writings range from unpolemical and understanding (when the Jews are seen as types of

Christ in an exegetical context) to negative (when contemporary Jews are depicted as avaricious infidels).

In part two Giles Constable analyzes Hildegard's commentary on about half the chapters of the Rule of St. Benedict, the only such text written by a woman. In response to specific queries sent to her, Hildegard emphasized moderation (seeing silence as "inhuman and boring" [p. 184]) and the adaptability of the rule to modern times. Franz Felten examines in great detail the social composition of Benedictine houses, concluding that Hildegard's exclusion of nonnoble members does not reflect the traditional social mix favored by the Benedictines. He also shows that later redactors toned down some of Hildegard's letters that dealt explicitly with some of the more questionable rituals involving the ostentatious splendor displayed by her nuns. Constant Mews provides a context for accusations against Hildegard's exclusivity by studying the ideals of the contemporary *Speculum virginum*, which did not champion the individuality favored by Hildegard. Yet, as Mews makes clear, this text had no direct influence on her writings.

Part three opens with Michael Zöller's study of different genres (revelation/apocalypse; allegorical vision; summa; and sermon) that could be helpful for classifying Hildegard's works. Peter Dronke investigates the limits of language as they appear in her *Ignota lingua*, while Bernard McGinn makes a splendid case for Hildegard as a "visionary exegete." Jean-Claude Schmitt contrasts Rupert of Deuz's vocabulary of visionary activity with that of Hildegard and Elisabeth of Schönau, concluding that male visionaries were permitted to dream, while female ones had to eschew dreams in favor of waking visions in order to shore up their authority. John van Engen investigates Hildegard's public persona as it was constructed in her letters, showing along the way that Pope Eugene III's letter, supposedly authorizing Hildegard's visions, was in fact a later forgery, probably by her assistant and scribe Volmar.

Irmgard Müller and Gundolf Keil offer two very technical studies of the significance of one particular manuscript (MS Laur. Ashb. 1323) for the reconstruction of the *Physica*; and of the vernacular (German) reception of the *Tractatus de herbis* that was linked by two redactors to the German *Macer* and thus survived in about 140 manuscripts.

The last part, on the reception of Hildegard's works, contains a meticulous investigation (the result of thirty years of research) by Albert Derolez of the manuscripts of Hildegard's visionary works. A number of tables visualize the connection and codicological characteristics of the manuscripts, with the Rupertsberg manuscripts constituting the most important group. Hans-Joachim Schmidt shows how each later mention of Hildegard's works actualizes a new meaning (see, for example, the compilation of her "prophecies" by Gebeno), how her image was reshaped, appropriated, and exploited in different periods (e.g. for pro- and

anti-mendicant propaganda). Laurence Moulinier shows us such a later Hildegard defending her works in a conversation with the devil, as part of a fifteenth-century French *vita* whose sources the author investigates in detail. Michael Embach explores the many ways in which Johannes Trithemius (1462–1516) propagated Hildegard's works and cult as well as his efforts to link her more closely to Sponheim. Marc-Ailko Aris, finally, moves into the nineteenth and early twentieth centuries, showing how readings of Hildegard were determined by the confessional background of modern Catholic or Protestant authors, by whom she was presented variously as a venerable prophet or as part of the superstitious dark ages.

These essays of invariably high quality represent a major contribution to Hildegard studies. They revise received opinions and provide a wealth of new material, from bold hypotheses to meticulous manuscript studies. No reader will put down this hefty volume unrewarded.

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MAUREEN C. MILLER. *The Bishop's Palace: Architecture and Authority in Medieval Italy*. (Conjunctions of Religion and Power in the Medieval Past.) Ithaca: Cornell University Press. 2000. Pp. xv, 307. \$49.95.

Maureen C. Miller has positioned herself in the midst of two current interests in medieval history: power, and architecture and language. Skillful in both fields, and possessing a magisterial knowledge of archival and printed sources as well as current literature, she has written a book that is required reading for anyone working on central and northern Italy. This study is both analytical and deeply probing and does not hesitate to contest the accepted canon, impelling historians to reconsider positions previously not thought to be open to dispute.

In part, Miller concentrates on central and northern Italy because it is the region of her expertise, but also because it is an area generally not covered by traditional accounts of medieval politics. Joining other historians such as Thomas Bisson, she focuses on the diverse ways in which power was exercised, experienced, and represented rather than on governance and the state. She steps back from a developmental history of power to appreciate its meaning and practice. Her aim is to recover the manifold workings of power by looking at cultural capital as well as governmental bureaucracies, and at losers as well as winners.

To determine why the bishop's palace became a source of power and splendor by the late twelfth century, Miller traces his residence back to the *episcopium* in the late antique period and the *domus sancte ecclesie* of the early Middle Ages. In the mid-eleventh century, these structures gave way to the *palacium*, a development that she associates with the Gregorian reform, which promoted higher moral standards for

clerics and the freeing of bishops and abbots from secular authority.

She argues that bishops lost temporal authority to the nascent communes, but rather than conceding defeat, they continued to believe that the church should influence, if not control, public life, and they sought innovative means to retain their power. Placing new emphasis on their cultural and spiritual presence, bishops compensated for their loss by competing with the communes for visual and spatial dominance within their cities. Architecture became one of their most effective resources.

They also invoked their spiritual authority to repress heresy and other deviations to gain an advantage over the secular strength of their rivals. Bishops rather than the communes were the oppressors, persecution their means. They effected a growing intolerance and repression in the Western church that eventually produced the climate that created the Inquisition. In sum, Miller argues that the weakening of the bishoprics created by the rise of the communes led bishops to resort to extraordinary lengths to retain their power. Repression and the creation of the elegant palaces to counter the commune's spaces were their primary means. She documents these changes with a wealth of photographs and skillfully drawn figures.

Although the subtitle of her book speaks of "authority," Miller fails to distinguish clearly between "power" and "authority." Among many other examples, in her conclusion she notes that the bishop used his spiritual power to buttress his authority, whereas arguably she might have reversed the terms. In *Duo Sunt*, written in 494, Pope Gelasius I asserted that the world was ruled by the sacred authority of the priesthood and the royal power. Implying that the "*auctoritas*" was greater than the "*potestas*," he said that responsibility of priests was more weighty because they would answer for the kings in the divine judgment. Since the distinction between these terms was widely discussed in the Middle Ages, Miller might have indicated more precisely what she meant by them.

Although she places the reason for the origin of the palace squarely in local political rivalries between the bishopric and the commune, she also correlates its development with outside phenomena. She observes, for example, that the eighth-century forgery, the Donation of Constantine, used by popes during the reform to justify their claims to secular as well as spiritual authority, first called the papal residence the *palatium* instead of its previous designation as the *patriarchium*. She notes that many bishops began using the term "*palatium*" during the Gregorian reform, but she draws no comparisons with the papacy and rejects the conflict between the emperor and the pope as a causal factor.

In sticking steadfastly to her conclusion that local fluctuations in power produced the transformation of the bishop's residence into a palace, Miller may have been too quick to reject other possible factors. But the

power of her arguments, and the elegance of her scholarship, command great respect.

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ANGELIKI E. LAIOU and ROY PARVIS MOTTAHEDEH, editors. *The Crusades from the Perspective of Byzantium and the Muslim World*. Washington, D.C.: Dumbarton Oaks Publishing Service. 2001. Pp. vi, 297. \$48.00.

Despite the massive and continually growing bibliography concerning the Crusades, and the changing historiographical approaches—long favorable but more recently critical because of associations with aspects of colonialism—carefully laid out by Giles Constable in his introductory chapter, the overwhelming majority of studies of this phenomenon continue to be focused on its Western aspects. The purpose of the symposium convened at Dumbarton Oaks by Angeliki Laiou and Roy Parvis Mottahedeh, of which the results are published in the present volume, was to compensate this unilateral perspective by a consideration of the attitudes and reactions of Easterners, be they Christian or Muslim, and the transformation of the economy and culture of the Eastern Mediterranean area, primarily of Byzantium, in the twelfth and thirteenth centuries under the impact of the Western expeditions and conquests.

Following the historiographic introduction, fourteen chapters (due to a particularly distinguished galaxy of specialists) address the theoretical and the more tangible aspects of these problems. First come the definitions of the nature and legitimacy of the "Holy War," whether the more specific and broader meaning of "crusade" or the alternate interpretations of the Muslim *jihād*—defensive and optional as against perpetual, aggressive, and obligatory—of which the latter was generally to become normative. Simultaneously, George Dennis argues for the absence of this concept in Byzantium, whose wars, in his opinion, were "imperial" and "just" but not "holy." The longest section of six essays is devoted to the perceptions and attitudes of Muslims toward Europe, as reflected in the Arab Epic Cycles, or toward Byzantium as found in twelfth and thirteenth-century sources; of Armenian Christians toward the Crusaders, colored by memories of apocalyptic prophecies and mythological pacts; and, most of all, of Byzantines toward Westerners. Particularly valuable are the chapters by O. R. Constable on the presence and precise development of institutions primarily intended for the use and control of Muslim and foreign merchants; by Laiou and Cécile Morrisson on the provisioning of the Crusaders on their way to Palestine, with the contemporary currency and alterations in the rate of exchange; and by David Jacoby on the economic transformation after 1204 of the Frankish Morea and the Venetian possessions, primarily in Crete. These illuminate the growth of specialized social and commercial institutions, the fragmentation

of great estates, the waning of the importance of Constantinople, the shift to a buyer rather than seller economy, and especially the redirection of trade and the insertions of Frankish Greece in the developing long-range commercial system of the Mediterranean dominated by Italian and primarily Venetian merchants. In opposition to the major impact of Western developments on the economy of the East, the last section finds only fragmentary and relatively insignificant manifestations of alterations in Eastern art that can be related directly to the presence of the Crusaders, although Oleg Grabar hypothesizes a "concentration of creative energy in the twelfth century wherever one looks" (p. 244), of which the Crusades were a part and of which the most brilliant syncretism would manifest itself in Norman Sicily.

The nature of the symposium format mandates a certain limitation of discussion and a fragmentation precluding uniform approaches or a unified development. Occasional disagreements are natural under the circumstances. Nevertheless, certain traits are to be found in a number of the essays. Most welcome is the absence of generalizations and oversimplifications in favor of a cautious and subtle argumentation. Elements of collaboration as well as hostility between the various groups are brought out. The radical opposition between Byzantium and the "Latins," particularly in matters of doctrine, is replaced by the recognition that the schism between the two churches did not exist at times or has been overemphasized. In particular, Tia Kolbaba argues in her chapter that notoriously disputed points such as the use of azymes for the Eucharist, papal primacy, or even the addition of the "filioque clause" to the creed developed as part of internal Byzantine arguments over the definition of Orthodoxy rather than as aspects of the hostility between Rome and Constantinople; even after 1204, Greek resistance to the Latin church could be merely passive and compromises possible (p. 129). In opposition to undifferentiated stereotypes, the late Alexander Kazhdan stresses that the "single unifying term *Latins*" appeared as a generic ethnonym as opposed to a linguistic designation only in the mid-eleventh century, and that Byzantine authors did not confuse the various ethnic groups present in the West, which they conceived until then as "composed of separate territories and distinct peoples" (pp. 84–86). As for the ethnonym "Frank," it was normally used after the eleventh century to designate the Normans of Sicily. Kazhdan's tracing of the distinguished careers of numerous Westerners in the Byzantine Empire during the second half of the eleventh century demonstrates further that some degree of *modus vivendi* between the two societies could be achieved. Similarly, a contemporary poem in praise of the emperor Manuel I Komnenos, discussed by Elizabeth Jeffries, distinguishes between the German contingent of the Second Crusade, which it attacks, and the French, of which there is no mention. Jeffries further points out that the author's extreme hostility toward the German Em-

peror Conrad III is transmuted into jubilation in his epithalamion for the marriage of Conrad's brother to Manuel's niece (pp. 108–109, 114). On the Muslim side, Mary Lyons warns against "a generalized identification in respect to Islam," since "contemporary Arab accounts" as well as "popular narratives" show no "united response" to the threat of the Crusades (p. 50). The surprising absence of hostility toward Byzantium in twelfth and thirteenth-century Arab sources is stressed, and the negotiations for an alliance between Isaac II and Saladin are duly noted (pp. 157–58). The appearance of Western chivalric concepts and romances in the Frankish Morea as well as of lexical innovations in thirteenth-century Greek speak to the presence of Latin influence, yet Sharon Gerstel also notes the earlier development in Byzantium of the Akritic Romances and argues that the massive appearance of military equestrian saints in the village churches in the southern region of the Mani after 1274 might be interpreted as a symbiosis of their apotropaic presence in the earlier Byzantine tradition and the emulation and assimilation of Western chivalric forms (pp. 273–80).

The attention given to the shift of geographical focus from west to east is of particular interest. The redirection of trade away from a focus on Constantinople, whose impoverishment and depopulation after 1204 no longer made it a center for the consumption of goods, clarifies the economic transformation of Frankish Morea and its pivotal role in the pattern of trade routes linking Italy to the East. Of equal interest is the transfer of Muslim primary attention from Constantinople to Jerusalem following the stalling of their apocalyptic expectations of world domination after the eighth century and more particularly as the result of the early Crusader focus on the Holy Land. That the persuasive argument of Byzantine polemics against use of azymes for the Eucharist had its origin in disputations with the Armenians being reintegrated into the empire as a result of Byzantine reconquests comes as a useful reminder that Eastern concerns were often as important, if not more, than Western ones for Byzantium.

A few reservations can be voiced in the midst of appreciation. All aspects of such a far-flung study could patently not be addressed, and Laiou explicitly states that "We neither expected nor planned a comprehensive examination" (p. vii). Nevertheless, a few additional aspects might well be desired. This is particularly true regarding the imbalance vis-à-vis the Islamic world. A more extended analysis of Muslim sources and points of view would have been most welcome. No study of Muslim attitudes toward the Crusaders accompanies that of their perception of Byzantium, no consideration of social or economic transformations in the occupied lands in the East. An analysis of the economic role of the Cilician kingdom, whose port of Ayas or Laiazzo became the only Christian one open to Westerners after the fall of Acre and provided the gateway for further transshipments

to Mamluk Egypt and Mongol Central Asia, would be useful. Occasional doubts arise on particular points. The argument that Byzantium did not have a concept of "Holy War" scants the religious aspect of Heraclius's campaigns, culminating in the return of the relic of the True Cross to Jerusalem in 629; of Nikephoros II Phokas's admittedly unsuccessful attempt to have the soldiers fallen in the Arab wars recognized as martyrs by the church; and especially of John I Tzimiskes's letter to King Ašot III of Armenia, preserved by the chronicler Matthew of Edessa, in which the emperor describes his expedition to Syria and Palestine as the "many wondrous things God has done for us and the astonishing victories he has given us," so that Ašot "might know how much God had aided the Christians at all times" and his own "intent on delivering the holy sepulcher of Christ our God from the bondage of the Muslims." Although in no way detracting from the argument that Byzantine polemics made against use of azymes in the eleventh century was first directed against the Armenians rather than the Latins, the book's chronology would benefit from greater accuracy. Armenian lands did not become part of the Byzantine Empire in the tenth century (pp. 122–23) but in the first half of the eleventh. The first Armenian kingdom ceded to the empire was that of Vaspurakan in 1021–1022. On a minor note, although recent events sadly have made the meaning of *jihād* all too familiar, nonspecialists might welcome a definition of such technical terms as *muqarnas*.

These however are but minor cavils; the explicit purpose of the books to redirect our attention to the East and stimulate "further research on developments that engage the eastern Mediterranean" (p. vii) has been amply fulfilled.

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BEVERLY MAYNE KIENZLE. *Cistercians, Heresy and Crusade in Occitania, 1145–1229: Preaching in the Lord's Vineyard*. (York Medieval Press.) York: York Medieval Press. 2001. Pp. xix, 256.

How did medieval Europeans come to treat "the Other" in such vitriolic and inflammatory ways? Why is the word "crusade" so objectionable to non-Europeans? Where did European anti-Jewish, anti-Muslim, anti-heretic, and antiwoman sentiments, as well as the justifications for the medieval Inquisition, come from? Medievalists have been struggling for some time with the problem of where to locate the origins of the Crusades, the Inquisition, and the justifications for increased persecution in the twelfth and thirteenth centuries. This book provides some extremely useful answers. That they are provided by Beverly Mayne Kienzle, an expert on medieval Latin preaching who has published widely on Cistercian sermons and related texts from the twelfth century, makes it more likely that the author has come up with useful answers

for why monastic leaders reacted so flagitiously to the asceticism and anticlericalism of the Cathar heretics. In so doing, she adds to our understanding of the origins of an intellectual tradition that still haunts us because of the hatreds it has created.

Kienzle not only tells the story of Cistercians drawn into preaching against heresy in southern France, but she carefully sets out the monastic context for the sermonizing against heresy to show how specifically monastic concerns about nonmonastic rivals for popular esteem led Cistercians to adopt four rhetorical strategies in their denunciation of dissidents: to demonize, to charge them with pollution, to present them as a threat to the social order, and to denounce them in apocalyptic terms, in a rhetoric that did not at first directly advocate violence but that created the opportunity for those rhetorical flourishes to be developed into denunciations that sparked violent attacks against dissidents within Europe as well as crusades against Muslims and pogroms against Jews. Kienzle's analysis of the sermons—the central part of the book—is to my mind so useful, so revealing, so brilliant that whatever other qualms I have about the book become minor quibbles.

When turning to the creation of context, Kienzle is less sure, and her use of sources usually consulted by historians does not weigh their reliability sufficiently. She does not differentiate the eyewitness accounts from those written at second or third hand, and events and miracles reported in the *Life of Bernard of Clairvaux* are taken at face value, without considering how much that text was rewritten to secure his canonization. There are also what seem to me two major gaps in the context constructed: the lack of reference to women other than Hildegard of Bingen, and an insufficient local Cistercian context.

Cistercian nuns are nowhere considered—yet houses of Cistercian nuns were founded by Fulk of Marseille, as abbot of le Thoronet, before he became bishop of Toulouse, specifically to provide a refuge for women who might otherwise enter Cathar communities. The foundation of Prouille, the first community of Dominican nuns, was similarly a house of religious women founded for those reasons with the assistance of said Fulk, bishop of Toulouse. One wishes that Kienzle had done more on the discourses of sermons with regard to the association of women with heresy—which seems to me a very puzzling development of the mid-twelfth century. Less important, perhaps, is that the documents for thirteenth-century Cistercian houses of women in northern France—founded by the Crusade families Montfort, Mauvoisin, and Marly, who made a number of gifts to those nuns as they were "about to depart on Crusade to fight the Albigenians"—explicitly identify the heretics as Albigensians, never as Cathars.

This is a story almost entirely of Cistercian abbots and monks from outside the region coming in to preach against heresy. The local Cistercian context—one in ten Cistercian houses by 1200 was located in this

region—is given little consideration, or is misunderstood. My work on the region suggests that the dichotomy drawn by Kienzle between Cistercian monks in the wildernesses of the countryside and Cathar heretics in the cities is too strict with regard to the many ties that Cistercians did indeed have with cities. Kienzle also misses the major point of my 1986 study: that Cistercians in southern France did not acquire uncleared, never before cultivated land, but land from which inhabitants were removed by the monks and nuns. This was done in some places rather gently, elsewhere in ways that were violent enough to have resulted in the murder of a lay brother. Cistercians, unlike other monks, moreover, appear to have been less responsive to the needs of surrounding populations; they did not provide hospitality for travelers but discouraged pilgrims, and by and large they did not provide alms or miracle-causing with relics for the sick. Their asceticism may have been great, but its savings were invested in more land, not in charitable activities. Houses like Fontfroide (from which several of the local preachers came) were notorious for their takeover of other, rival hermits and monasteries. Does the failure of outside Cistercians to convert heretics back to orthodoxy reflect a failure, for instance in charitable activities, of the local Cistercians?

There are little things to criticize in the book, most notably the abysmal photographs, including one on the jacket that is mislabeled; readers will want to know that the citations to the *Exordium Magnum* are from chapter seventeen, but book two of that text. Nonetheless, a book on the tie between the Cistercians and the crusade against the Albigensians in southern France has been needed for a long time; in providing it, Kienzle has also given us something even more important: an analysis of justification of persecution in the medieval church that has consequences for our own time.

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ANTONY LEOPOLD. *How to Recover the Holy Land: The Crusade Proposals of the Late Thirteenth and Early Fourteenth Centuries*. Burlington, Vt.: Ashgate. 2000. Pp. x, 231. \$79.95.

Nearly thirty recovery treatises—memoranda suggesting ways in which the Holy Land might be recovered and held by Latin Christians—were written between 1290 and 1336. These were not the earliest. Four survive from the 1270s, and several dating from even earlier must have been lost. But they provide a fascinating insight into European responses, often at a very senior level, to the loss of the crusader beachheads in Palestine and Syria. They belong to a much larger corpus of treatise writing, but their lifespan was quite short. After the 1330s, Western Europe was subjected to the Hundred Years' War and the Black Death, and the increasing need to defend Europe itself against the Turks took precedence over the ambition to retake

Jerusalem. The treatises began to attract attention in the late nineteenth century, but it is only in the last thirty years that they have become the subject of more intensive study by Kenneth Meyer Setton, Sylvia Schein, Christopher Tyerman, Norman Housley, and others.

No one, however, has looked at them as comprehensively as Antony Leopold. He considers the authors, their treatment of preparations for a new crusade and theoretical justifications for it, their strategy (in particular, the planning of preliminary smaller scale expeditions, which would test the ground and wear out the enemy), their proposals for the organization of a general passage and for the government of the kingdom of Jerusalem once it had been reconquered, and their influence in the longer term. This is certainly the best-researched study of the memoranda written so far. Leopold is interesting on the contacts between the authors, their relations with enthusiastic patrons like Pope Clement V and a succession of French kings, and their influence on the court of Burgundy in the late fourteenth and fifteenth centuries. But although he clears the ground for future research, his book is not definitive. It is, first of all, very descriptive. Leopold does not really rise above what he reads and give it wider meaning. For example, he mentions in passing that at about the same time as memorialists were proposing that a future kingdom of Jerusalem should be governed by a military order, the Hospitallers and Teutonic Knights were establishing their own order-states in Rhodes and Prussia, but he does not develop the relationship between events on the ground and theory, in spite of the importance in history of these and other theocracies such as Malta and the papal patrimonies.

This may stem from the fact that Leopold is not very knowledgeable about the wider subject. He cannot be blamed for following Schein and Housley's picture, built on Georges Digard's errors of the 1930s, of Philip IV's disinterest in the Holy Land (whereas Philip, in fact, revived his grandfather's practice of financing a significant French force in Palestine). But he does not seem to realize that the refusal of European kings to allow their subjects to take the cross was an ancient tradition and not a new development. He is shaky on crusade ideas and on the continuing power of the penitential element in recruitment. He does not appear to understand that Ayas in Armenia provided a way round the embargoes for merchants and that James de Molay's arguments against a preliminary landing in Cilicia stemmed from the Templars' experience as the guardians of the Belen Pass, which, if held properly, barred entry into Syria through the Amanus mountain chain. He underestimates the strength of the arguments for an overland crusade. It was not simply hatred of the Greeks that led to six memorialists arguing in its favor. The proposal had been revived in Pope Gregory X's great project of the 1270s, and the seriousness with which it was still being discussed suggests that there really was something in it.

And the idea of reaching the Holy Land by way of North Africa was a much longer held idea than Leopold supposes. It first surfaced in 1125 and reached a culmination in the bitter struggle for North Africa in the sixteenth century. He does not relate the discussion of castles in Palestine to the pressing need expressed by the memorialists for a standing army, which was a much more important issue than the size of garrisons.

Nevertheless, in spite of its deficiencies, this is a valuable work. It provides the basis for much future research.

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KENNETH FOWLER. *Medieval Mercenaries*. Volume 1, *The Great Companies*. Malden, Mass.: Blackwell. 2001. Pp. xiv, 384.

In the decade that followed the Anglo-French peace that ended the first phase of the Hundred Years' War in 1360, Western Europe suffered from a surplus of armed men who lived by war. The peace between the kings left many combatants officially unemployed, turned out of the fortifications and towns they had occupied, and set loose in the countryside with little or no provision for their maintenance. To many in unfortified towns and villages—and even to inhabitants of some major fortified centers—the peace must have seemed more perilous and daunting than the war. Many of the warriors formed into *routes* (often comprised of men from one country or *pays*) that collectively came to be known as the Great Companies, a term they used themselves. It is their story that Kenneth Fowler tells in this “thick narrative” that obviously draws upon several decades of work and utilizes the entire range of evidence available: modern scholarship, and printed and manuscript sources. This book will surely stand as the standard history of the companies for generations to come.

Fowler advances their story along a chronological axis, with successive chapters taking a particular theater or personality as a focus. Although there was no overall organization or leadership, each *route* followed a captain likely to be of a chivalrous, often even noble family; most were young men on the make; not a few were bastards. For many of them, it was a fatal undertaking: roughly ten percent of the captains died in battle, and likely more than ten percent were executed.

The companies looted, burned, and extorted ransoms from individuals and communities across many provinces of the kingdom of France, even threatening the pope in Avignon; they moved across the Pyrenees to take part in the wars between Aragon and Castile-Leon and were even involved in plans for Sardinia before returning north to ravage parts of France once more. The dilemma they forced upon licit authorities appears in the two general policies attempted by the

papacy: the companies were declared excommunicated and crusade preached against them, yet they were also recruited for crusades directed into the Balkans and against the Moors in the kingdom of Granada. Lay authorities similarly found it necessary both to guard against them and to employ them, sometimes simultaneously. Populations and resources were regularly hurried into strongly fortified centers along any route likely to be taken by the companies, whether they came as paid allies or as outright looters and brigands. Since any pay promised them was almost always in arrears, their looting and demands for “protection money” were constants, whatever formal arrangements might theoretically be in place with local or national authorities.

Just as the peace of 1360 had created the conditions in which the Great Companies emerged, so resumption of the great Anglo-French war reabsorbed them in large measure into the royal armies resuming their work. Of course, by this time some leaders and many rank and file were dead and some companies had broken up. In a remarkable move by the French King Charles V, Bertrand du Guesclin, a famous leader among the companies, had become constable of the French royal army. More of the companies, however, had probably served the interests of the English, specifically the Black Prince, rather than the kings in Paris. This comes as no surprise, since many of the fighters had been recruited for the French war in the southwest of France or in the northern counties of England.

As with any “thick narrative,” this book offers the benefits of watching events unfold with all the complexity and contingency any historical story shows when told in detail. The reader will sense how powerfully the Great Companies entered into the political and military history of this decade. Frequent, excellent, full-page maps help greatly. The sheer devastation and misery Fowler narrates in campaign after campaign will likewise register with all who read his book. Perhaps some, in company with this reviewer, will experience disappointment that we must wait for a deeper analysis of the mass of evidence presented; this is promised for a second volume. Exactly where the companies went, what their numbers likely were, how they devastated the land, and what contracts they made with “legitimate” authorities we now have before us in the present volume. What all this comes to in the history of violence, law making, statebuilding, and commercial enterprise in Western Europe is yet to come. The issues are no less fascinating and important than they are difficult.

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STANKO ANDRIĆ. *The Miracles of St. John Capistran*. New York: Central European University Press. 2000. Pp. x, 454. \$49.95.

This book by Stanko Andrić is primarily a scholarly analysis of the manuscripts of miracle collections produced for John of Capistran's canonization campaign. John of Capistran (d. 1456), an Observant Franciscan, was not only a popular preacher but also played a key role in the defense of Belgrade against the Ottoman Turks at the end of his life. After the Christian victory, John, gravely ill, made his way to Újlak (Ilok) to die. The shrine at Újlak became the center of a cult, where miracles proliferated. Seven miracle collections were compiled between 1460 and 1521. Andrić's chief merit is in establishing the authorship and circumstances of composition of each collection through an exhaustive analysis of the manuscripts and an examination of their textual relationship. With great erudition, he takes the readers through his reasoning step by step, giving detailed proofs both in the text and in ample appendixes. The bulk of the book consists of a technical study of the manuscripts and an investigation of the narrative structure of miracle accounts.

While one can only admire his work at a philological and technical level, Andrić is much less successful in providing a broader historical analysis. He sets the scene through a competent summary on the Franciscan Observance and John of Capistran (correcting some mistakes in previous scholarship, e.g. about John's origins) and a short presentation on the town of Újlak. But, apart from brief suggestions, he only tackles the problems of a historical analysis of miracles in the last chapter; even here, he gives a mostly quantitative analysis of types of miracles and categories of beneficiaries. His work on narrative structures is thorough but leads to rather predictable conclusions, mostly confirming the results of scholars such as Pierre-André Sigal, Ronald C. Finucane, or André Vauchez. Sometimes Andrić proposes thoughtful modifications of analytic frameworks, such as a distinction between distance-dependent and distance-indifferent miracles. At other times, naïve "arguments" mar the text, contrasting sharply with Andrić's sophistication as a scholar of manuscripts. For example, although perceptively analytical of hagiographical topoi, Andrić nonetheless assumes that in *vita* miracles mirror the personality of the saint rather than being textual constructions. His conclusion, "Towards a Unified Theory of Miracles," engages with the question of the reality of miracles, mixing insight and flirtation with the supernatural.

The real lack in the book is an in-depth analysis of the genesis of the cult itself. One would especially like to know more about why Nicholas of Újlak, one of the most important magnates in the kingdom, who styled himself king of Bosnia, promoted John of Capistran's cult, making sure first that the saint, or his body, would not leave his town, and that it would be buried "*honorifice*" in the convent where his own ancestors' graves were. Nicholas arrived at the battle of Belgrade belatedly; was he trying to stake out an alternative claim to be a defender of Christendom through the

possession of at least the body of what he hoped was a soon-to-be canonized saint? Andrić barely explores the relationship between politics and religion; similarly, beyond a fleeting mention, he does not explore the difference in the shrine's popularity among the Hungarian as opposed to the Slavic population. Neither does he address the admittedly not miraculous but nonetheless intriguing transformation of John of Capistran from a pugnacious antiheretical and anti-Ottoman preacher and crusader to a stereotypical healing saint. Despite both Andrić's assertion that it is possible to establish a continuity between John's lifetime activities and his posthumous miracles, and (allegedly) John's and (certainly) his promoters' clearly expressed ideas about the role of the dead body continuing the work of the saint in bringing about conversion and protecting the community, one is struck by the essential lack of continuity. The only point of connection, possibly making the dead John of Capistran "an anti-Ottoman crusader and defender of Christendom" (p. 305) as the living one had been, is through John's miracles of liberation from captivity. Even here, however, the link is tenuous, as liberation from captivity represents only 4.6 percent of all miracles: that is, twenty-three cases, and out of these, only fourteen deal with liberation from Ottoman captivity. The vast majority of miracles are exactly the same type of healing miracles that were produced by other medieval saints.

Despite the book's shortcomings, Andrić's analysis of the process of compilation, and his edition of selected miracle stories, are important contributions to the study of late medieval sanctity.

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EUROPE: EARLY MODERN AND MODERN

STEPHAN R. EPSTEIN, editor. *Town and Country in Europe, 1300–1800*. (Themes in International Urban History, number 5.) New York: Cambridge University Press. 2001. Pp. xi, 343.

Stephan R. Epstein has commissioned and edited fourteen essays that investigate relations between towns and the countryside in early modern Europe. In his introduction, Epstein provides a useful survey of the main issues in this field and explains this book's general approach. The book concerns small towns as well as big ones and takes a functionalist approach to defining what constitutes an urban or country setting. The theme of all the essays is urban political economy and the constraints placed on it by other sources of authority, mainly the state. Earlier town-based theories of economic development have given way to models that emphasize agriculture as the engine of early growth. Many of the authors take note of the Brenner Debate, but it is not clear what consensus, if any, has emerged from the clashes of the Neo-Malthusians and Neo-Physiocrats. These authors prefer an

institutional approach, and so they look for urban and national powers that hindered or fostered market integration, both between cities and between cities and their regions.

Epstein's own findings, which cover most of Europe, suggest that urbanization is a very poor predictor of economic development. The essays on Castile (one of the most urbanized regions) by Pablo Sánchez León and Sweden (one of the least) by Robert Sandberg add weight to this startling conclusion. The two essays on early modern Holland, another highly urbanized region, explain the Dutch Republic as an accident of state formation and its relative economic stagnation as a result of tax policies over which cities had too little control. There is a great deal of evidence across Europe for Epstein's view that coercive institutions like states and guilds affected economic performance. Rent seekers, those who used political mechanisms to foster their own economic status at the expense of others or the common good, shaped local political economies, usually for the worse, especially in the countryside.

A final unifying theme of the book is the complicated issue of proto-industrialization. This theme unifies urban and rural histories because the authors generally agree that much of proto-industrialization begins as a rural phenomenon and becomes more urban over time. The mechanism for how this happens is unclear, but the authors tend to follow Epstein's line that restrictive guild practices push some innovation out to rural areas, where new techniques establish themselves. This part of the story is familiar, but how these rural practices come back to the cities is not always clear. Perhaps Paul Glennie in his essay on town and country in England from 1570 to 1750 provides the best answer by looking at the new towns like Birmingham and Manchester that literally arose in rural settings. An older established city like Florence, a place where some advanced techniques in cloth manufacture had existed from the Middle Ages, may have been, as Carlo Belfanti suggests, hostile to innovative techniques. Florence's domination of its countryside was so thorough as to preclude proto-industrialization there, but English cities did not possess that kind of coercive authority over smaller towns and the countryside. The explanatory model that emerges from these comparisons is the search for coercive institutions.

These essays accomplish the difficult task of summarizing what in most cases is a vast literature on the topic of cities and countryside. In a few areas, as Andrzej Janeczek points out for the Polish Commonwealth, and Brigitte Marin for the Kingdom of Naples, more work needs to be done on early modern cities in places where rural studies have predominated. Epstein has orchestrated a volume that is truly European in scope. The one glaring omission concerns the substantial part of Europe ruled by the Ottoman Empire. An essay on Balkan cities would have yielded fruitful comparisons with the rest of Europe on the issues of

state power, taxes, and coercion. The chronological sweep of the book is also impressive, but the authors do not always seem in agreement on what constitutes the early modern period of European history. Epstein himself covers the years 1300 to 1800, but few of his authors follow this ambitious course. The most curious exception concerns Germany, for which Tom Scott has written an essay that stops in 1600. Marjolein 't Hart's excellent essay on the Dutch Republic carries her story down to 1800, so one natural comparison between these two regions and their cities becomes impossible for the reader to make.

The book's rigorous sense of what constitutes political economy prevents the authors from introducing some themes that might have provided a broader context for their work. The authors have worked hard to dispel the idea of studying cities apart from their regions, but there is not much interest in agriculture here. Only Thomas Brennan, in his essay on France, pays special attention to this topic, and this must result from his own fine work on viticulture. More striking still is the complete absence of the colonies and their effects on the ways early modern cities and rural areas developed. The Europe in this book is enclosed from global influences. But some European cities, like Amsterdam and London, benefited directly from the fruits of empire, and others, like Madrid, rose on the wealth derived from very distant countrysides. Early modern merchants were capable of envisioning how to exploit agricultural possibilities on Barbados or to extract wealth from the cities of India. Readers will need to think about how any approach to issues like coercion and proto-industrialization can succeed by completely leaving out topics like slavery and the sugar business. Also, how can we explain the divergent paths to development experienced in different parts of Europe without reference to the colonial ventures?

As Epstein astutely points out in his introduction, traditional approaches to the discipline of historical geography have shaped the ways scholars in different parts of Europe have studied cities and their regions. One example of this phenomenon is the way Johann Heinrich von Thünen's theory of urban-rural interaction influenced the way some German scholars study the topic. Yet von Thünen rings have appeared in studies outside Germany, and Adam Smith's views on guilds commanded wide respect. This volume was not influenced by recent interest in the field of historical ecology and its emphasis on microregions. The divisions of Europe here are the traditional national or provincial ones. A fresh look at political economy might include a stronger dose of political ecology and ask questions about how very local environmental and cultural factors also influenced the pace of development. The interconnectedness of the microregions, and a global perspective, might have helped to frame these valuable essays.

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PATRICK O'BRIEN *et al.*, editors. *Urban Achievement in Early Modern Europe: Golden Ages in Antwerp, Amsterdam, and London*. New York: Cambridge University Press. 2001. Pp. xiv, 361. \$64.95.

There are three reasons why this volume has a coherence often lacking in edited collections. First, it focuses on just three mercantile imperial cities: Antwerp, Amsterdam, and London. Second, each of these had its golden age roughly in succession, meaning that the analysis is of "high points" in metropolitan development: respectively *ca.* 1492–1585, *ca.* 1585–1659, and *ca.* 1660–1730. Third, the volume seems to have been a long time in gestation and it has been thoroughly edited to address its goal, which is to answer the question: "Why do recognized and celebrated achievements, across several fields of endeavour, tend to cluster within cities over relatively short periods of time?" (p. 5). Five areas are studied for each of the three cities: economic growth and demographic change; architecture and urban space; fine and decorative arts; books and publishing; scientific and useful knowledge. Patrick O'Brien's thoughtful and wide-ranging (if episodic and occasionally discursive) introduction explains the origins of the book in a major collaborative, interdisciplinary (really multidisciplinary) research project funded by the Renaissance Trust.

The fifteen remaining contributors (two of the editors have not contributed chapters of their own) are nearly all well known and highly regarded academics, most of whom work in Britain and the Low Countries. Most are economic and social historians in the broadest sense, plus historians of art, architecture, and literature. Nevertheless, they all show an awareness of the political (including military) context in which the three cities experienced their respective moments in the sun. The three economic and demographic chapters dwell principally on overseas trade: the demographic analysis is confined to migration and size, with little information on fertility, nuptiality, or mortality. Michael Limberger and Clé Lesger see economic success in regional rather than purely urban terms. Because London was many times bigger than Antwerp or Amsterdam, Peter Earle focuses more on the purely metropolitan economy. Turning to urban space, Piet Lombaerde shows how Antwerp "provided a unique site for the practical expression of the ideas typical of Renaissance, mannerist and early baroque architecture and town planning." His chapter catalogues what was built and by whom. Marjolein 't Hart, an economic and social rather than an architectural historian, goes further both by setting Amsterdam building in the context of public and private initiatives and by comparing that city with Antwerp and London. Judi Loach's discussion of late Stuart and early Hanoverian London is enjoyable, even if the conclusions do not take us much beyond what T. F. Reddaway and John Summerson wrote decades ago. The best of the chapters on the fine and decorative arts is Marten Jan Bok's on Amster-

dam. Punchy and original, it is also one of the most stimulating in the book. The three chapters on books and publishing address the central theme of why these cities should have been so successful. Antwerp's publishing and book trade is studied by Werner Waterschoot, principally through discussion of prominent figures and those associated with them (for example, Christophe Plantin). The scholarship is good, but, except for four pages on festivals, ritual, and the performing arts, the approach is dated. Paul Hoftijzer's chapter takes a slightly different line when dealing with Amsterdam's publishing. He does not toward Robert Darnton's methodology, but again the result is a predictable outline of predispositions and personalities. In one of the shortest but most pleasing chapters, Karel Davids makes a rare attempt to compare the social context of England and the Netherlands when explaining why Amsterdam was a center of learning.

This is a good book where the individual chapters all manifest high quality scholarship and the overall realization is successful. Aware of theory, the authors stick reassuringly to positivism and attempts explicitly to establish or follow "models" are few (e.g. pp. 17, 250). The text is handsomely produced, and its appearance is enhanced by two dozen text illustrations. There is, however, an underlying tension, which detracts from the integrity of the book. Given its origins, the areas used for comparison are understandable. Nevertheless, they represent a traditional and frankly value-loaded "golden age" view of what are social and cultural achievements, and they do not allow for the contradictions and failures that exist among all of history's apparent successes. O'Brien acknowledges both of these points (pp. 5–6, 10, 35). The contributors (to their credit) sometimes chafe against them, notably Limberger when he questions outright whether the polarizing effect of economic growth on wealth was really beneficial to sixteenth-century Antwerp. Furthermore, many of the chapters spend their early pages setting the scene. This may have been necessary at the conference stage, but it is perhaps superfluous if the market for the book really is specialist urban historians (p. 33). Indeed, one senses that all concerned were torn between the dictates of initial funding, their professional interests (or perhaps consciences?), and the exigencies of securing a publisher.

The contributors and editors would probably agree that other yardsticks could be used for comparison. These include literacy, where some important new cultural work finds no mention. (An example is Erika Kuijpers, "Lezen en schrijven: Onderzoek naar het alfabetiseringsniveau in zeventiende-eeuws Amsterdam," *Tijdschrift voor Sociale Geschiedenis* 23 [1997]: 490–522.) Jan De Vries and A. M. van der Woude's book is cited only in the 1995 Dutch edition (*Nederland 1500–1815: De eerste ronde van moderne economische groei*), not the 1997 English translation. Other measures include the consumption of everyday material goods rather than just conspicuous and expensive art and artefacts (a topic addressed directly, if briefly, only

by Earle), the extent of political participation, and the status of women. Finally there is social structure and the distribution of wealth, for the way a society deals with its poor (and indeed its sick) is arguably the ultimate benchmark by which its "achievements" should be gauged.

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MARTIN RHEINHEIMER. *Arme, Bettler, und Vaganten: Überleben in der Not 1450–1850*. (Europäische Geschichte.) Frankfurt: Fischer Taschenbuch. 2000. Pp. 252. DM 21.90.

In recent years, a new form of historical writing has emerged and proliferated. A curious hybrid, neither research monograph nor textbook for introductory courses, these works function, according to their marketing, to present findings in various subfields to advanced undergraduates and beginning graduate students. Like a textbook, in other words, these books rest upon the work of dozens of other scholars—often far beyond the research expertise of the author—and are intended to sell to large numbers of readers. Unlike a textbook, they take up "themes," themselves defined very much by contemporary scholarship, its foci of research, its internal debates, its specialized vocabularies, and its particular historicity.

Martin Rheinheimer's book is such a hybrid. Rheinheimer has published articles on gypsies in Schleswig-Holstein and on the poor in Großolt, 1700–1900. This book, however, claims to treat the categories of "the poor," "beggars," and "vagrants" for all early modern Europe and to describe how various people survived "need" between 1450 and 1850. Rheinheimer draws, therefore, upon other works, although largely restricted to those in German. Missing, for example, are important studies in English such as Linda Martz's and Maureen Flynn's work on Spain, Brian Pullan's work on Venice, and Thomas Max Safley's work on Augsburg.

Rheinheimer's volume reflects the problems of such hybrids. Frequently, the arguments of research scholars, made directly from their sources and within specific contexts, become generalizations. Rheinheimer is aware of that danger but nonetheless falls into it. While he begins by recognizing that poverty is not the same in all times and all places, within the same paragraph he then cites statistics for "begging in the seventeenth century" (p. 14). One wonders how those statistics were derived; no endnote gives the source for this data or the specificities of the subject population. Beggars are a notoriously difficult segment of any population to measure: begging itself had become variously criminalized in both Catholic and Protestant lands by the seventeenth century, so those who begged, both resident and foreign, were elusive of authorities, ecclesiastical or secular.

The greatest danger of this sort of book, it seems to me, is that the categories research scholars work out in

specific contexts, with precise qualifications, acquire a quiddity not present in their original application. While Rheinheimer opens with a list of kinds of poor defined according to persistence of need (categories much closer to early modern legislative distinctions), in the actual division of discussion he treats women in a chapter separate from "the true poor," the next chapter. Early modern authorities recognized the particular financial vulnerabilities of widows but certainly viewed them as "true poor." As Rheinheimer's own anecdotes illustrate throughout, moreover, they made no gender distinctions among "false beggars," "vagrants," or other kinds of "undeserving poor." Unsurprisingly, Rheinheimer's chapter on "gypsies" is the most attentive to differentiation and nuance. There he can draw on his own research to refine and illustrate the distinctions he wishes to make.

Rheinheimer's stated focus is the various strategies different poor adopted in order to survive. Important and salutary, Rheinheimer's focus also suffers the piecemeal effect of dependence on monographs: while he provides a number of rich anecdotes, drawn from his own data and that of others, he can offer no overview of strategies for survival among, say, women throughout early modern Europe. Was prostitution the constant and consistent response to poverty between 1450 and 1850, in Catholic Madrid as well as Reformed Leiden? Was infanticide a "normal" response or exceptional? Such problems go to the heart of this kind of hybrid.

In German, this work is not easily accessible to American undergraduates, who, according to the most recent statistics, are increasingly monolingual. For scholars of early modern Europe, it offers no new data, no new thesis. Its emphasis on the increasing importance of property in social distinctions is interesting but ultimately unproven. Can the increase in the percentile of those who owned feather pillows be, as Rheinheimer takes it, a measurement of prosperity or of increased production, and the consequent reduction in price, of a commodity?

Such a hybrid leaves one at the end with the question: to what are students introduced? Not to the application of reflected historical methods to an articulated body of sources. Perhaps to some sources, but they are often twice mediated derived from other scholarship. Sometimes to historiographical questions, but often, as here, without any discussion of how those questions themselves are historically grounded. What is the "history" these books present?

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OLE PETER GRELL and ROY S. PORTER, editors. *Toleration in Enlightenment Europe*. New York: Cambridge University Press. 2000. Pp. ix, 270. \$59.95.

JORIS VAN EIJNATTEN. *Mutua Christianorum Tolerantia: Irenicism and Toleration in the Netherlands: The Stin-*

stra Affair 1740–1745. (Studi e tesi per la storia della tolleranza in Europa nei secoli XVI–XVIII, number 2.) Florence: Olschki. 1998. Pp. viii, 355. L. 64,000.

The murderous ethnic conflict in the Balkans during the 1990s has fueled a renewal of interest in the European history of toleration. This notion was a fundamental but polymorphous ideal of the Enlightenment, and these two very different books usefully add to the growing literature on its embattled articulation and fitful application during the seventeenth and eighteenth centuries. One emerged from a conference held at Cambridge in 1997 and encompasses both a wide range of historiographical approaches to the topic and a rapid but thorough survey of the varied interpretations of toleration in different national settings. The other, a detailed monograph on a little-known controversy in Friesland in the early 1740s, meticulously situates the “*Stinstra Affair*” within the wider context of Dutch toleration debates in the late seventeenth and early eighteenth centuries.

In their introductory essay, editors Ole Peter Grell and the late Roy S. Porter rightly emphasize the fragility, ambiguity, and contentiousness of the concept of toleration in eighteenth-century Europe, and the limited and varied nature of its application across the continent. Their exploration of this complexity valuably deflates any glibly linear notion of an inexorable and straightforward advance of toleration, neatly nested within the forward march of Enlightenment. However, this recognition also leads to the risk of collapse into relativism: a pitfall that vividly haunted many of the early advocates of toleration discussed in this volume. Grell and Porter do not address this tension head-on, and their essay therefore oscillates somewhat precariously between a mutedly heroizing reconstruction of the tolerationist arguments of pioneering philosophers and polemicists such as John Locke, Baruch Spinoza, Pierre Bayle, and Voltaire and a more neutrally contextual emphasis on the pressures of pragmatism and statecraft in shaping the contours of toleration in the various jurisdictions of Europe. This tension also runs through the volume as a whole, which encompasses both explicitly argumentative and methodically synoptic contributions, as well as several attempts to span this rift. However, this appropriately ecumenical diversity is perhaps the greatest strength of the volume. The study of toleration by historians has tended to be somewhat isolated from parallel debates on the subject among philosophers and political scientists. While this collection is very much driven by historical concerns, it does not shy away from the abstract issues that cannot be evaded in any general consideration of the nature and scope of toleration.

The first few chapters of the collection take a more transnational and philosophical approach to this theme. Robert Wokler advances a trenchant defence of the “Enlightenment Project,” which, he claims, was centrally committed to the values of multiculturalism and pluralism. In contrast, Sylvana Tomaselli argues

that tolerance was very seldom celebrated as a positive virtue in itself during the seventeenth and eighteenth centuries: it was more often advocated as a means to an end. Martin Fitzpatrick offers an account of an essentially unitary Enlightenment campaign for toleration, spearheaded above all by the philosophes of Paris. Jonathan Israel, however, discerns a sustained tension between the theological “Arminian” tradition of toleration theory, culminating with Locke, and the more radical “republican” theory, most cogently advanced by Spinoza, which argued not merely for toleration among Christian denominations but for freedom of thought *tout court*. Each of these essays is animated by a distinctive intellectual standpoint and offers a contrasting reading of similar material. Taken together, these differences are extremely stimulating, and stage in microcosm the diversity of approaches that continue to enliven the study of the Enlightenment.

The remaining chapters are devoted to eight distinct national case studies. These essays again differ markedly in approach, but they are all of an extremely high standard and combine to provide a wide-ranging, if selective, tour of both the theory and practice of toleration in much of eighteenth-century Western and Central Europe. Marisa Linton, on France, and Ernestine van der Wall, on the Dutch Republic, emphasize the imbrication of the concept of tolerance in debates on virtue, nationhood, and citizenship in the later eighteenth century. Joachim Whaley elucidates the unique complexity and diversity of interconfessional arrangements in the Holy Roman Empire from 1648 onward. Pragmatic exigencies, he argues, shaped and sustained these toleration regimes. Both Whaley and Karl Vocelka, on the Habsburg monarchy, concur that the extension of toleration in the enlightened absolutist legislation of the later eighteenth century addressed the question of toleration much less systematically or extensively than traditional mythologies suggest.

On Italy, Nicholas Davidson vividly dissects the pragmatism and compromise that characterized both the repression of the Inquisition and reformists’ ultimately victorious campaign for its abolition. Michael Müller argues that the limited toleration in eighteenth-century Poland-Lithuania was more to do with the association of non-Catholics with foreign powers and alien cultures than with any peculiarly backward rejection of modernity, while Henry Kamen stresses the fact that the monoconfessional nature of Spain in this period left the question of toleration there largely moot. The inclusion of these three chapters seems in part intended to decenter traditional contrasts between the Enlightenment’s supposed core and its periphery. However, Kamen’s Spanish account in particular tends to reinforce this distinction. The nation-based pluralism of the volume reflects the current trend toward the fragmentation of the Enlightenment, influentially exemplified by Porter’s earlier volume, coedited with Mikuláš Teich, *The Enlightenment in National Context* (1981). While this approach very

valuably draws into consideration areas often neglected by intellectual historians, its primary weakness is that it obscures the self-conscious internationalism of discussions of toleration within the "Republic of Letters." This volume tells us a great deal about the differences between concepts and practices of toleration across Europe, but it leaves to the reader much of the work of thinking through how they interconnected.

Almost all the essays in the volume largely focus on interconfessional relations internal to Christianity. Islam is scarcely mentioned, let alone other non-European religions. There is no chapter on Russia, or any coverage of toleration in colonial contexts, either of which would have conceptually enriched the collection. Judaism, too, is surprisingly little discussed, with the notable exception of Justin Champion's essay on England, which fascinatingly explores the connections between John Toland's 1714 advocacy of the naturalization of the Jews and the rumpus over the "Jew Bill" of 1753. Toland's argument, Champion shows, was based on a radically nontheological, Tacitean understanding of the civic utility of tolerance. This mode of argument remained indigestible in England forty years later, as the swift repeal of the "Jew Bill" demonstrated. Unlike the other "national" contributors, Champion does not provide a swift survey of the contours of toleration in Britain but instead offers a richly stimulating microstudy, interweaving high politics, radical philosophy, and cross-cultural intellectual exchange and stereotyping.

Johannes Stinstra makes only one fleeting appearance in Grell and Porter's volume. However, this Mennonite minister from the small Frisian town of Harlingen provoked a controversy that resonated across the Dutch Republic in the early 1740s. His radical arguments for unrestricted freedom in faith and worship drew the opprobrium of the Dutch establishment and led to his suspension as a preacher on the grounds of suspected Socinianism. This episode has been largely forgotten by historians—a situation that is amply rectified by Joris van Eijnatten's monograph. Van Eijnatten provides a detailed contextual analysis of the dispute, followed by the complete text, in its original Dutch and in English translation, of the *Deductie* (1740)—the argument, in all probability by Stinstra, that triggered the controversy. He also appends, in Latin only, the most authoritative refutation of Stinstra: the brief *Oratio* (1745) of the conservative Leiden theologian Johannes van den Honert.

Stinstra's arguments, van Eijnatten shows, drew on a strong Dutch tradition asserting the autonomy of the individual in matters of conscience. He was indebted to the jurists Gerard Noodt and Jean Barbeyrac, as well as, perhaps, Bayle and, from England, Benjamin Hoadly and Locke. However, the Dutch concept of toleration, even as advanced by these theorists, remained highly ambiguous. A civil notion of permitted pluralism sat awkwardly alongside a more ecclesiastical understanding of toleration as forbearance in the higher interest of the preservation of concord. Almost

all leading authorities regarded Socinianism as beyond the bounds of the Christian commonwealth on which this concord was based. Stinstra's opponents were thus able to argue that these same writers lent support to their condemnation of him.

Readers of this study may legitimately conclude that scanty awareness of the Stinstra affair has not much diminished our understanding of the history of toleration. Van Eijnatten himself acknowledges that this debate was similar to parallel discussions in England and Germany, and that Stinstra's adversary Van den Honert was "not an original theologian" (p. 91). However, he elegantly deploys this episode as an intellectual prism, through which the tensions and disputes in Dutch thinking on toleration are more clearly visible. Nonspecialists are unlikely to be gripped by this study, but scholars of Dutch intellectual history will glean much more from it than knowledge of the Stinstra affair alone.

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NIALL FERGUSON. *The House of Rothschild*. Volume 1, *Money's Prophets 1798–1848*. Volume 2, *The World's Banker 1849–1999*. Paperback edition. New York: Penguin. 1999. Pp. xvii, 518. \$14.95.

The name of Rothschild elicits superlatives: the richest family the Western world has ever seen; the bankers of prime ministers and monarchs, managing in their portfolios the policies of European states; a financial power of remarkable flexibility and durability, lasting for a century or more. There is much truth in all these superlatives, especially for the nineteenth century.

One might conclude that since Bertrand Gille's hefty two-volume *Histoire de la Maison Rothschild* (1965, 1967), there was little need for another history of the Rothschilds. What Niall Ferguson has done is bring to bear new material from British archives that was inaccessible to Gille. This material not only reveals in great detail the financial operations of the London branch, and indeed all branches of the Rothschild firm, but also the day-to-day family relations exposed in business and personal correspondence. The result is family history at its best.

We know that the five Rothschild brothers of the second generation worked together with remarkable harmony and efficiency, usually deferring to Nathan in London as *primus inter pares*. But until Ferguson we did not grasp the tone and texture of that cooperation. For example, there is something direct and forceful about James's observation to Nathan about Portuguese bonds: "We have a great many asses who have been buying this shit" (I, p. 356). This is not the same persona we see in countless caricatures of Nathan's profile standing in morning coat and top hat at the London stock exchange. Not that Ferguson is concerned only with uncovering "*les Rothschild chez eux*." On the contrary, he is at great pains to chart each

financial operation with abundant historical context, perhaps too abundant for the average European historian. But one can only praise Ferguson for his thoroughness and his exhaustive use of the correspondence of the entire family network.

Ferguson emphasizes the boldness and diversity of Rothschild financial operations, especially when they reached full stride in the 1840s. Beginning during the Napoleonic Wars, the London branch became the indispensable banker and paymaster of the British armed forces. It was the Rothschild bank that provided the Duke of Wellington's army in Spain with pay and supplies, a service the general did not forget after the war. One is impressed by the scale of financial operations and by the delivery system, which depended on tight cooperation of the five brothers across Europe. This was a private system of speedy information and rapid transport by land and sea, and of key contacts ranging from harbor police to ministers of state. From acting as paymasters the Rothschilds moved to negotiating British government bonds (consols). Their great contribution to modern finance was the creation of internationally tradable, fixed-interest bearer bonds, making it possible, for example, for British capitalists to invest in the debts of other states and receive their interest in London. The Rothschilds also developed the technique of profiting from small changes in the rates of exchange which often brought in more revenue than straight commissions and interest. Equally important, as the firm expanded its loans to governments, it gained temporary access to public funds, which it invested short-term on its own account like the tax farmers of the Old Regime. At this stage in their history, the Rothschilds were on the margins of court banking and modern public finance.

The Rothschilds did not run their family bank on political principle. After the Napoleonic Wars, James in Paris made loans to the restoration government, Solomon in Vienna lent money to Prince Metternich, Amschel in Frankfurt continued to lend capital to the German principalities, and Carl dealt with the Neapolitan Bourbons. After the Revolutions of 1848, they shifted to the new regimes with little difficulty. Indeed, they responded to new economic developments with investments in railroads in France, Prussia, and the Habsburg Empire; mercury in Spain; copper in Russia; sugar, tobacco, and cotton in the West Indies and Latin America; and later gold, silver, and diamonds in South Africa and oil in Russia. The commodities were carefully chosen. In retrospect, their only big mistake was failing to invest in the United States after the Civil War, but the Rothschilds, like many Europeans, were very wary of the exuberant and erratic American business milieu. In all of these operations, the family functioned as a single unit, reaching decisions by common accord but clearly guided by Nathan until 1836 and by James until 1868.

Ferguson dispels a number of myths about the Rothschilds, especially those relating to speculative coups such as buying up British consols before the

news of Waterloo arrived in London or harboring the personal wealth of the elector of Hesse-Kassel during the Napoleonic occupation. Ferguson does not deny that these speculative maneuvers took place, but he maintains that they did not play a major role in making the Rothschild fortune. That was the result of patient and careful day-to-day decisions over the long run. True, there were dramatic moments such as the loan to the French government to pay off the indemnity to the new German Empire or to Benjamin Disraeli to buy the Suez Canal shares, but the Rothschild fortune was built on the constant reinvestment of the surplus in a century when there was no income or inheritance tax.

How did the Rothschilds spend their money? Not all of it was plowed back into the firm, even in the generation of the fabulous five. Not many Rothschilds remained long in the tight Jewish quarter of Frankfurt with feisty grandmother Girtle Schnapper Rothschild. Wealth brought *éclat* and status. One of the best examples of their social arrival is the painting by Sir Francis Grant of Nathan's sons Lionel, Anthony, Nat, and Mayer de Rothschild riding to hounds in crimson hunting attire. Other pictures and daguerreotypes capture important moments in the life of this dynamic Jewish family. Consider Lionel, duly elected member of Parliament, refusing to take the Christian oath of admission to the House of Commons, a defiance he repeated on four occasions until Jewish emancipation finally came in 1859; the first Lord Rothschild (Natty) lounging casually with top hat cocked jauntily on an English lawn with Randolph Churchill; or the marriage of Charlotte and Anselm von Rothschild in 1826, the cousins seated and surrounded by senior Rothschilds in serried ranks. By the 1840s, four of the five Rothschild brothers had their manor and town houses, staffs of servants, French cooks, fine equipage, landscaped gardens, stud farms, Bordeaux vineyards, and botany and art collections, and—by the late century—a university education, usually Cambridge, for their sons.

Did their energy and entrepreneurial skills endure? With the exception of Amschel at Frankfurt, the Rothschild brothers had large families and so did their children. There was always one male in each family who had business acumen, at least until the fifth generation, permitting his siblings to pursue other interests. Many of these were serious pursuits. Edmund devoted much of his money and energy to establishing settlements for Jews in Palestine; James's wife Betty, painted by Jean-Auguste-Dominique Ingres, hosted a Parisian salon; others were art collectors, botanists, horse-breeders, educationists, and philanthropists—and not exclusively for the Jewish community. Some were more frivolous. Adolphe and Louise built a lavish château on Lake Geneva and did little else; Walter trained wild zebras to pull his carriage. The Rothschilds spawned their share of eccentrics, not all English. Family discipline was strictly maintained in other ways. First, marriage within the family was expected by the family patriarchs; of twenty-one Rothschild marriages between

1824 and 1877, fifteen were among cousins, usually first cousins. Second, marriages outside the Jewish religion were strictly forbidden; Hannah's marriage to Henry Fitzroy was a terrible travail for Hannah. Finally, the women and sons-in-law in the family were excluded from all business decisions. To be sure, there was some breakdown in these strictures by the early twentieth century. But marriage into the British aristocracy had its advantages for family status and wealth and was reluctantly accepted by the family elders.

Ferguson raises the question of Rothschild political power, a constant theme in the anti-Semitic press. In the early nineteenth century when the Rothschilds had public finance almost to themselves—Baring and Pereire were far behind them in the volume of banking operations—their decision to loan or not to loan to a major state such as Prussia or Russia could affect prospects for European conflict. But, insists Ferguson, the Rothschilds also had to survive; they were as much hostages to state policy as its creator. In addition, as the century progressed the various branches of the Rothschild family, like everyone else, became more nationalistic. Surely by the eve of World War I they did not see themselves as a cosmopolitan alliance, much less an international Jewish plot, but rather as a loose confederation of cousins willing to fight and die for their respective fatherlands. The dissolution of the international partnership in 1905 was indeed a sign of the times. Yet even today something of the spell of the Rothschild name must remain for them as it does for us. Will a single family ever again so dominate the international economy and reach the pinnacle of prestige among the governing elites of Europe?

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J. W. BURROW. *The Crisis of Reason: European Thought, 1848–1914*. (The Yale Intellectual History of the West.) New Haven: Yale University Press. 2000. Pp. xv, 271. \$29.98.

J. W. Burrow's book is the second volume to appear in the Yale Intellectual History of the West series. In addition to possessing many other merits, it follows well-chosen methodological principles. First, Burrow is attentive to those whose fame did not outlast their own time rather than writing a book on "what is living in late nineteenth-century thought." Second, he renounces the aim of writing general summaries of the views of his thinkers, preferring instead to emphasize material that fits into his broader thematic choices. His book does not constitute adequate introduction to the thinkers he treats but carries out excellently his aim of providing a comprehensive context. Third, he pays just the right amount of attention to national intellectual traditions without departing from a basically thematic approach. In spite of rising nationalism, this was an age in which intellectual influence was more broadly international than ever before.

Burrow writes with wit, unfailing clarity, and astute observation. Few names are dropped without a penetrating comment, few subjects left unilluminated, and his illustrations are rare and imaginative. His thesis is that the rise of materialism and a growing historical sense left the sense of personal identity highly vulnerable and made self-creation, with or without strong nationalist identification, both difficult and imperative. The result was a highly varied proliferation of outlooks in which artists, social thinkers, and social activists attempted to fill a spiritual void with increasing shrillness and decreasing confidence as time went on. The merits of the book, however, lie less in this reasonable thesis than in its details.

Occasionally, one can quarrel with Burrow's interpretations. It is sound to see John Stuart Mill's *On Liberty* (1859) treated as part of a rising cult of aesthetic individuality; it is misleading to overlook Mill's echo of John Milton's point that freedom of expression is essential for the discovery of truth, which neither author conceived to be a merely individual matter.

Far more serious is what Burrow tells us about an underrated figure, Thomas Hill Green, and what he does not tell us about an overrated one, Friedrich Nietzsche. Green, he writes, "left the mundane common good too far behind. Altruism of Green's kind is a hot potato which can be passed on but which no one consumes" (p. 132). This overlooks the depth of Green's insight into the crisis of his time. Green emphasized a universal self-realization of moral consciousness because the alternative was the idolatry of purely competitive goods, and this idolatry was both hard on "the less favoured members of society" and must lead to a continuation of social life as "war" (*Prolegomena to Ethics*, sect. 244, 1883).

Green's point is the indictment of his age. The "Prometheism" and the inversion of G. F. W. Hegel by Ludwig Feuerbach and Karl Marx, with which this book begins, begged the crucial question not raised by Burrow: exactly why should the dignity and power to be reclaimed from God by the human race not be reclaimed by individuals in a war of all against all? When Darwinism demoted consciousness to the status of merely another tool for survival (a demotion Burrow notes), the result was the destruction of traditional notions of a teleology for consciousness that could link human beings together, at least in moral, religious, or philosophic theory. This did more than shake oppressive authority. The very concept of a socially intelligible meaningfulness that was more than the production of goods on the one hand or the triumph of brute power, which could seem "noble" by comparison with commerce, on the other, became threatened. This in turn produced an increasing respectability for violence as an exercise in individual or national self-proving, to which Burrow refers far too briefly, and, among some, a cult of ruthlessness of which we get an insufficiently vivid impression, even though the author's treatment of Social Darwinism is in many ways typically insight-

ful. It is in connection with these points that Burrow's sparse and abstract reference to Nietzsche's immoralism, which conceals its cost, is damaging. Instead of citing "For the Englishman morality is not yet a problem" (p. 171), he might better have quoted the following prescription: "The essential characteristic of a good and healthy aristocracy . . . is . . . that . . . it accepts with a good conscience the sacrifice of untold human beings who, *for its sake*, must be reduced and lowered to incomplete human beings, to slaves, to instruments" (*Beyond Good and Evil*, sect. 258).

The tendency to obscure the brutalities punctuating Nietzsche's thought is common enough. But its repetition in this admirable book is particularly unfortunate. Burrow has set forth most of the intellectual elements of the age he treats, but he has not fully expressed the overwhelming intensity of its moral drama. Fortunately, for that one can still go to *The Magic Mountain*.

JOHN ANDREW BERNSTEIN
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NORMAN M. NAIMARK. *Fires of Hatred: Ethnic Cleansing in Twentieth-Century Europe*. Cambridge: Harvard University Press. 2001. Pp. 248. \$24.95

The politicization of ethnicity and the radical reduction of diverse forms of identity into unidimensional ethnopolitical categories are among the most widespread and characteristic features of the contemporary era. The consequent systematization of violence against ethnic minorities forms the subject of this wide-ranging, comparative study.

Norman M. Naimark brings a sensitive and humane vision to bear upon some of the twentieth century's most unspeakable manifestations of state-initiated intolerance and brutality. In limiting the scope of his coverage to the European continent, broadly defined, he leaves himself a more than adequate range of cases to choose from. Beginning with the state-coordinated mass slaughter of Armenians in World War I-era Turkey, the author charts a course through the subsequent Greco-Turkish "population exchange," the Jewish Holocaust of World War II, the mass deportations of Chechens and Crimean Tatars in the wartime Soviet Union, the postwar expulsion of Germans from Poland and Czechoslovakia, and on to the most recent cases of ethnic cleansing in the former Yugoslavia. Indeed, one of the main purposes of the book is to place these latest outbreaks of state-sponsored interethnic violence in historical perspective.

Drawing on a representative and up-to-date sample of the secondary literature in the major European languages, as well as on recently available Russian primary sources, Naimark presents a concise, synthetic historical narrative about each of his selected cases. The individual stories he tells are not fundamentally new, but the point of his book is to bring them into a comparative, analytical framework. In so doing, he is careful to avoid moral oversimplification. Naimark

stresses that all cases of violence against ethnic groups are not equivalent, either in their motivation or in their scope and intensity. He suggests that the events he discusses can be placed along a spectrum ranging from ethnic cleansing to outright genocide, and, indeed, that some cases of ethnic cleansing are more understandable in their motivation than others. For example, the Czechs' postwar drive to expel the Germans of the Sudetenland is hardly surprising in light of that population's record of overwhelming support for the Nazis who had so recently dismembered and occupied the country. Yet even in this case, the reductionism of ethnic politics led to many anti-Nazi activists among the Czechoslovak Germans suffering the same fate as their pro-Hitler countrymen. Moreover, the process of expulsion was not neat and orderly but abusive and violent, with tens of thousands of civilians perishing in consequence.

Given that the morally least questionable instances of population transfer have been indiscriminate in practice and have resulted in pillage, rape, and murder on a massive scale, the author takes a strong stand against the sanctioning of ethnic cleansing under any circumstances by international law. The 1923 Treaty of Lausanne set a dangerous historical precedent, he contends, by lending an aura of legality and respectability to the expulsion of nearly 1.5 million Greek Orthodox people from Turkey and over 350,000 Muslims from Greece—a violent and brutal demographic upheaval that had largely run its course by the time the treaty was negotiated. The myth of Lausanne as a legal framework for peaceful population exchange helped shape the Allied agreement at the July 1945 Potsdam Conference sanctioning the ongoing removal of ethnic Germans from Poland and Czechoslovakia. While the West's recent diplomatic and military interventions in the Bosnian and Kosovo wars have nominally been undertaken in defense of the idea of the multiethnic state, the effective consequence of those interventions has been territorial partition along ethnic lines. Each such case, Naimark argues, only increases the chances of future ethnic violence being initiated by manipulative political elites or vengeful irredentist movements.

This book vividly depicts the moral, human, and political costs of radical ethnonationalism and emphatically underlines the primary role of the modern, homogenizing state in institutionalizing the politics of group hatred and engineering the most sinister forms of ethnic cleansing. The chapters on the expulsion of Germans from East Central Europe and the Soviet transfers of Chechens and Tatars are particularly finely textured and reflect the author's deep familiarity with the relevant sources. The chapter on the Jewish Holocaust could stand on its own as a well-informed synthesis of the recent literature, transcending the simplistic functionalist-intentionalist dichotomy to present a convincing picture of the etiology of the Nazis' campaign of mass extermination while also highlighting those aspects of motivation and implementation that lent the Final Solution its distinctive

character as the most extreme and irrational of recorded genocides.

Beyond the audience of interested scholars, the book would serve very well in courses on European politics, modern nationalism, and the uses of state power. A note of caution: while the author goes to pains to distinguish the nature of the Jewish Holocaust from the other instances of genocide and ethnic cleansing he discusses, a superficial read by an inexperienced student could lead to a facile lumping together of these varied forms of persecution into an undifferentiated moral and political category. In other words, this material would serve more effectively as the basis for a critical classroom discussion than as a textbook assigned for background reading. This is simply a function of the complex subject matter and not of the quality of the author's intentions or analysis.

Greater care in the production of this book by the editorial staff of Harvard University Press could have been expected. The decision not to include any of the accent and diacritical markings for Slavic and Turkish names is lamentable. A few minor errata could have been eliminated: the standard dates for the Ottoman Tanzimat reforms are 1839–1871, not just the 1860s (p. 20); the nationalist “Memorandum of the Serbian Academy of Sciences” was published in 1986, not 1996 (p. 149), as is evident from the context of the author's own discussion.

On the conceptual front, it would have been interesting to include more discussion of the practical alternatives to the sanctioning of ethnic cleansing in countries riven by ethnic warfare, as well as an examination of the relationship between ethnic homogeneity and successful democracy in ethnically cleansed states such as Poland and the Czech republic. But to point this out is to attest to the stimulating and thought-provoking nature of this book, which will be of value to anyone interested in the politics of identity in the modern era.

AVIEL ROSHWALD
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EDWARD E. ERICSON III. *Feeding the German Eagle: Soviet Economic Aid to Nazi Germany, 1933–1941*. Westport, Conn.: Praeger. 1999. Pp. xiii, 265. \$59.95.

The relationship between Germany and Russia in the twentieth century has long fascinated scholars, and certain periods have attracted more attention than others. Of particular interest to scholars has been the period of collaboration inaugurated with the signing of the Nazi-Soviet Pact on August 23, 1939, and lasting until the German attack on the Soviet Union on June 22, 1941. Much of the previous scholarship on this period has focused on military and political relations, while economic relations have received less attention. Arguing that “economic ties were often more central to whatever friendship existed between the two states than their political or military cooperation,” (p. 1) the work under review here tries to redress this imbalance.

Edward E. Ericson III presents a well-researched, detailed analysis of the economic relationship between Germany and the Soviet Union during this important period of Nazi-Soviet collaboration. Echoing others, Ericson notes that Nazi Germany and Soviet Russia were in many ways natural economic partners. They had complementary economies: Soviet Russia could supply Nazi Germany with raw materials, and Nazi Germany could supply Soviet Russia with advanced technology. This fact notwithstanding, economic relations between Berlin and Moscow were anything but close during the early years of the Nazi regime. Adolf Hitler did not favor close relations of any kind with Bolshevik Russia. It was, Ericson argues, the worsening international situation in 1939 and the outbreak of war in September of that year that eventually motivated Hitler to overcome his hostility and enter into a serious economic relationship with Moscow. Soon to be at war with and blockaded by the Western powers, Hitler turned to Russia as a valuable source of raw materials vital to the German war economy.

The Nazi regime soon discovered that Moscow was a tough negotiating partner. Rejecting the interpretation of scholars who view Soviet policy toward Germany during these years as one of appeasement, Ericson repeatedly demonstrates that the Soviets drove a series of hard bargains and more often than not were able to achieve the advantage in their negotiations with Berlin. Yet Ericson convincingly shows that despite the tough and, at times, frustrating negotiations with the Soviets, there were individuals within the German Foreign Office who viewed the economic relationship with Russia as a useful one and who wished to see it continue. Moreover, the relationship was indeed useful for Germany, whose stunning victories in Western Europe in 1940 were, Ericson argues, made possible by raw materials supplied by Joseph Stalin, as indeed were the spectacular, and for the Soviet regime nearly fatal, victories won by the Wehrmacht in the opening phases of the Russian campaign in 1941.

Hitler, however, did not wish to see the relationship with Moscow continue. Following Germany's victories in 1940, the führer was increasingly anxious to realize his long-sought goal of conquering “living space” in the East. Indeed, Ericson argues, Germany's dependence on Russia for vital raw materials only strengthened Hitler's resolve to smash the Soviet Union in a military campaign, thereby gaining direct control of these same raw materials. Thus, for Hitler, economic relations with the Soviet Union were for short-term goals: the securing of strategic raw materials for the German war economy. Stalin, on the other hand, had longer-term goals in mind: the securing of advanced German technology, especially military technology. Moreover, by enabling Hitler to fight in the West, Stalin hoped to further his own diplomatic and military goals. This was a logical and sensible policy but one that fell victim to Murphy's Law, according to Ericson. Germany's unexpected victory in the West in 1940 enabled Hitler to turn his attention east much earlier

than Stalin had expected, and, as was the case in 1940, Soviet raw materials powered the Wehrmacht's initial eastern victories in 1941.

Based on extensive research in German archives and a careful and detailed analysis, this book does indeed demonstrate the centrality of economic ties in German-Soviet relations during 1939–1941. The book's only real weakness, although hardly a fatal one, is that in assessing Soviet motives, it relies solely on English and German secondary literature. None of Ericson's conclusions regarding Soviet policy are based on materials from Russian archives or on Russian secondary literature. Nevertheless, this is an important contribution to the history of German-Soviet relations during the era of the Nazi-Soviet pact as well as to the history of World War II in Europe.

J. DAVID CAMERON

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JOHN CONNELLY. *Captive University: The Sovietization of East German, Czech, and Polish Higher Education, 1945–1956*. Chapel Hill: University of North Carolina Press. 2000. Pp. xviii, 432. Cloth \$55.00, paper \$24.95.

In this detailed and well-argued comparative study, John Connelly asserts that university institutions of self-rule stood in the way of Communist desires to subordinate higher education to state planning in postwar East Germany, Poland, and the Czech lands. He contends that universities remained diverse throughout the Stalinist era. None of the three national university communities willingly acceded to Communist rule.

The end of World War II resulted in differing contexts for the reconstitution of the various professoriates. Polish academics, who had never surrendered their control of university affairs—indeed, who had continued to teach during Nazi occupation—considered it their right to reopen universities. This was not the case, however, in the Czech Lands or East Germany. In the former, Communist students played a major role in reorganizing the universities. In the latter, where the trauma of National Socialism and the lost sovereignty of the professoriate delayed the “new opening” of the universities, Soviet and homegrown Communist-inspired revolution from above gained a fairly complete break with the past. Although student opposition emerged in the early postwar years in all three countries, only in Poland did professors join students in defending university autonomy. Longtime university traditions and differing university milieux, as well as the divergent beginnings of postwar history in these three countries, adumbrated the differing experiences of Stalinism in the universities. While local Communists were dedicated to transplanting the Soviet university system in their various countries, they proved able to re-create only its external structures. Attempts to reproduce the internal life of Soviet universities met resistance that varied by country.

The second and third sections of the book—“The

Legacy of the Old Professoriate” and “The New Student”—constitute path-breaking contributions both to the comparative political history of Stalinism and to the history of higher education in the region. Connelly's treatment of the Polish experience is extremely thorough. Rather than focusing solely on the world-famous, tradition-rich Jagiellonian University in Cracow, the author examines the experiences of universities throughout the country, including the rebuilt Warsaw University and the “new” universities in western Poland. He dissects the political interplay among the various faculties as well as the thick web of personal and patronage relationships that connected academics throughout the country, enabling a cohesive professoriate to contest Polish Communist attempts to exert influence over academia in 1947. For the most part, the professors remained in place, frustrating Communist attempts to instill a new consciousness among peasant and working-class students. The partially transformed Polish universities produced both intellectual dissent and popular pressure, with university students among the most active opponents of the regimes throughout the Communist era.

Although the Czechoslovak Communists were able to remove “objectionable” faculty from the philosophical faculties of universities, working-class parents refused to make their children available for university education, stymying party attempts to form a student body in which working-class youth constituted a decisive element. As Connelly argues, Czech Communists failed to change working-class attitudes toward higher education because of their “own disregard for intellectual enterprise” (p. 288). Therefore, middle-class students retained positions at universities. In the Czech lands, too, university students actively opposed Communist regimes in times of conflict.

East German universities, particularly liberal arts and sciences faculties decimated by postwar denazification, were further shrunk by migration to the West, which stopped only with the construction of the Berlin Wall. Helped by the Soviet occupiers in keeping student numbers low, the German Communists were able to create a new student: one who was both politically committed and of the “correct” economic background (p. 272). Unlike their Czech and Polish counterparts, East German students seldom opposed Communist regimes.

This volume is somewhat uneven. The first part, “Sovietization,” is less clearly written than the other sections; the reader is provided too much information without enough analysis. There is some unnecessary repetition, perhaps because the book is organized chronologically and topically. There are occasional minor errors of fact as well as in the bibliography and endnotes. Moreover, inconsistent use of some geographic place names makes this book more difficult to read than need be. The author appears to use the terms “Central Europe,” “East Central Europe,” and “Eastern Europe” interchangeably throughout the text. The nonexpert might find it helpful to know when

and why the author employs Vilnius rather than Wilno or L'viv rather than Lwów. Finally, consistent first and last-name identification of the many historic figures introduced would be useful. More thorough editing could have corrected such infelicities in this important and otherwise well-written work.

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ANDY WOOD. *The Politics of Social Conflict: The Peak Country, 1520–1770*. (Cambridge Studies in Early Modern British History.) New York: Cambridge University Press. 1999. Pp. xvi, 354. \$74.95.

This is a richly documented and important book, difficult to discuss adequately in a short review. Wood provides a comprehensive and convincing account of the free mining communities in the Derbyshire Peak country from the late sixteenth to the late eighteenth century. His detailed, often moving regional study is combined with broad and methodologically sophisticated discussions of how best to understand early modern society, so that his book makes a most significant general contribution to our understanding of social identities, social protest, and economic change. In particular, he rehabilitates a regionally and occupationally specific notion of class as central to analysis of early modern society.

Wood's lead miners are not the inscrutable and disorderly men condemned by elite commentators but a community of skilled independent producers staunchly defending and indeed extending mining rights through effective exploitation of complex legal arrangements and carefully organized direct action. The book shows how rising demand and technological change from the later fifteenth century created the conditions for the dominance of "free miners," who claimed the right to dig for ore wherever it was found within the crown's Duchy of Lancaster Derbyshire Peak estates. Wood's stress, however, is on human agency within structures of inequality rather than on impersonal social forces. The free miners were economically dependent on rich ore buyers or smelters, but they retained control over their own labor through a piece-rate system known as "coping." The miners had a strong sense of the customary rights underpinning their access to the lead field, and they were often widely experienced as jurymen in the barmote courts that regulated the industry. In addition, the miners and their allies were often able to mobilize central courts, notably that of the Duchy of Lancaster, in their defense. The mining communities thus had an assertive plebeian culture activated by a vivid sense of the past—by a notion of custom that was continually contested and recreated, and indeed was capable of innovation, as with the recodification of mining law by the Great Barmote of Wirksworth in 1557. This culture was self-consciously masculine, with women confined to an auxiliary economic role and excluded from participation in the formal institutions of the industry.

Mining was a skilled, male activity, while caving for wages was feminized and dishonorable.

In the early sixteenth century, the miners were apparently quiescent politically, subordinate players in disputes amongst the gentry. By the end of the century, however, they had become more assertive in the defense and extension of their rights. In Wirksworth, the miners worked with the Earl of Shrewsbury (the duchy's tenant) against individual gentlemen who sought rights to the lead on their estates and obtained confirmation of free mining through the duchy courts. In the High Peak, however, Shrewsbury opposed the miners, and struggles over mining rights were part of a simpler social cleavage marked also by conflict over enclosure and access to common land. Legal sanction for free mining was strongest in Wirksworth, so in the first half of the seventeenth century High Peak miners argued for the extension of the practices there to the whole of the Derbyshire lead field. The miners exploited divisions among their social superiors, and their own leaders came not from the rich or the very poor but were men with "woolle of their backs" (p. 260).

Wood's account is based on extremely thorough research in local and central archives, including an analysis of 3915 depositions in duchy, exchequer, and church courts. A petition from almost 2,000 miners to the House of Commons in 1641 forms the basis for a thorough account of the mining communities at this pivotal moment, revealing how free miners were threatened by wage labor—"hirelings"—on the one hand, and large, capital intensive mines on the other. Wood does not accept the prevailing orthodoxy that social conflict become less severe in seventeenth century, and he offers an illuminating and complex account of how the assertive plebeian politics of the Derbyshire miners became tangled up with the dilemmas of civil war allegiance. In Wirksworth the miners bitterly resisted Attorney General Heath's attempts to take control of the major Dovegang works; they lost that battle but their stubbornness prevented Heath extending his methods elsewhere. In the High Peak, an unrelenting and inconclusive struggle with Sir John Gell over the lead tithe, conducted locally and through central equity courts, came to a head in the 1630s. These battles helped to determine civil war allegiance; against David Underdown, Wood stresses "it was not ecology which determined allegiance in the Peak . . . but rather the contingent interests of a large section of the mining workforce" (p. 269). Thus the High Peak miners were willing to support the king providing he lifted the oppressive lead tithes, and they certainly did not rally to Gell, Parliament's chief commander in Derbyshire. The Wirksworth miners, who had no quarrel with Gell, and no reason to love the royal court, did join Parliament.

The civil war encouraged a "radicalization and polarization" of the struggles in Derbyshire. Plebeian miners without gentry support continued to resist the attempts of the Manners, earls of Rutland, to exclude

them from their Haddon estates. Rutland had the backing of central legal bodies, of Parliament and its troops, while the miners seem to have attracted the support of the Levellers, the London-based democratic movement. Wood's evidence for the Leveller-miner links is more elusive than at other parts of the book, but his suggestions are at least plausible. Both miners and Levellers stressed the legal and political rights of male heads of households; both groups were bitterly disillusioned by Parliament's support in the 1650s for elite property rights and both, perhaps, were tempted by royalism. The miners lost the long battle with Rutland, and by 1660 wage laborers worked the Haddon fields. More generally after 1660, Wood argues, there was much starker social polarization in the Derbyshire lead country. Men of middling status (like William Bagshawe, who had opposed the lead tithe in the 1630s) now identified with elites and the central state. Lead prices collapsed, and the judgements of the duchy court were largely hostile to the miners. By the eighteenth century, miners had "a defensive but rebellious culture" (p. 313). They still used law and direct action, but to defend rather than extend their position, and their struggles were increasingly focused on wages rather than on the rights of independent workers.

Wood's account is sometimes repetitive and does not always make for easy reading, but it more than repays the effort required. The necessarily attenuated account of developments after 1660 lacks the subtlety of the earlier discussions, but in general Wood convincingly shows that the class consciousness of the industrial revolution was profoundly influenced by the stress on custom, locality, skill, and masculinity found among the earlier Derbyshire miners. Wood's book is a very fine example of the too rarely attempted integration of social and political history and successfully combines a fully realized regional study with a major contribution to our general understanding of early modern England.

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LAURA LUNGER KNOPPERS. *Constructing Cromwell: Ceremony, Portrait and Print, 1645–1661*. New York: Cambridge University Press. 2000. Pp. xiii, 249. \$59.95.

Oliver Cromwell's reputation among his contemporaries was almost as varied and controversial as that which was shaped and re-shaped in the centuries after his death. First as general and then as politician and Lord Protector during Britain's only direct experience of republican rule, he attracted enormous attention and endless disagreement. This book by Laura Lunger Knoppers, alludes, for example, to seventeenth-century biblical comparisons made between Cromwell and King Ahab, King David, Elijah, Gideon, and Moses, not to mention classical comparisons with *both* Brutus and Julius Caesar. Panegyrics and popular satire of the day extended the range even further. Knoppers's book

examines and tries to explain all this contemporary image making.

As a literary critic rather than a historian, and with a significant book on *Historicizing Milton: Spectacle, Power and Poetry in Restoration England* (1994) already to her credit, Knoppers is well placed to elucidate texts produced by writers as ill assorted as John Dryden, James Heath, Lucy Hutchinson, Andrew Marvell, John Milton, and Edmund Waller, as well as contemporary ballads and newspapers. But this book is about visual no less than verbal constructions of a national leader; Knoppers's sources also include paintings, engravings, and medals and they are dealt with in a painstaking and probing way. The famous and distinctively different portraits of Cromwell by Robert Walker (1649), Samuel Cooper (c.1650), Peter Lely (1654), and Edward Mascal (1657) are carefully scrutinized and firmly located within the particular context in which they were originally produced. But Knoppers is equally illuminating when discussing caricatures such as the bizarre, bulbous-nosed creation of 1654 (illustrated on p. 87) and the bold post-Restoration print of 1663, which reworked Walker's earlier heroic portrait by the simple addition of a noose around Cromwell's neck. The Dunbar medal of 1650 and the death medal of 1658 also receive attention that historians have usually failed to give them.

Cromwell himself, as Knoppers makes clear, "did not tightly control the production of his own image" (p. 3). This obviously left the field open to "a wide range of image-makers and image-breakers, including those who wished to remake him in the image of a king" (p. 6). Knoppers denies that Cromwell's self-representation was ever monarchical. Except on state occasions and when receiving foreign ambassadors, the Protector's preference, she says, was always for the "plain style." The Mascal portrait of 1657 (p. 131), the last one for which the Protector sat, is an emphatic contradiction of the conventional royal iconography of the day. Dignified in appearance certainly, but care worn and noticeably ageing, Cromwell in his plain collar and dark cloak looks more like a Dutch merchant than an English monarch. Knoppers offers a fascinating discussion of Cromwell's funeral that links its contrived ceremonial—effigy, lying in state, pomp and procession, and all the rest—with that used at the last royal funeral in England (James I in 1625). As Knoppers makes clear, however, all the arrangements were made by a new, legitimation-seeking Council of State. Other interesting comparisons are made in this thought-provoking book. That between Cromwell's two installations as Lord Protector and the coronation of Charles I is particularly instructive (pp. 107–12, 122–28) and pointedly underlines the absence of divine ritual in the ceremony and oath taking of 1657. The final "ceremony" that comes under discussion in this book is Cromwell's posthumous "execution," impeccably timed on January 30, 1661 to coincide with the anniversary of the dispatching of Charles I in 1649 on

the scaffold erected outside his own palace of Whitehall.

Excesses such as this, as Knoppers emphasizes, could completely backfire. Verbal, visual, and—in the 1661 incident—physical assault perversely drew attention to, and magnified, the very object of derision. Restoration abuse of Cromwell, in satire and engravings no less than in the grisly brutalities of January 30, 1661, became new acts of remembering the dead leader instead of acts of erasure and helped to keep his memory alive in the public sphere. Not the least of the many merits of this highly successful interdisciplinary study is that it helps us understand not only the complex processes of “constructing” Cromwell that went on during his own lifetime and immediately after but, by extension, his unstoppable afterlife down to the present day. In an opinion poll in the United Kingdom conducted in December 2001, many respondents unhesitatingly nominated Cromwell Britain’s greatest monarch.

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JOHN EGLIN. *Venice Transfigured: The Myth of Venice in British Culture, 1660–1797*. New York: Palgrave. 2001. Pp. x, 262. \$45.00.

The myth of Venice is among the most enduring political archetypes in European culture. Since the late fourteenth century, *la Serenissima*’s legendary stability was an object of curiosity for commentators across Europe. Over the succeeding centuries, intimations of the republic’s mortality paralleled Venetian self-glorification as the city became as well known for its courtesans and Carnival as for its mixed constitution or its extensive maritime empire. When Napoleon Bonaparte finally put the myth to rest in 1797, he consigned the polity to the realm of memory and the city to the sphere of aesthetic, leaving John Ruskin to record its “stones” lest the mistress of the Adriatic should disappear beneath the waves.

Venice’s grip on the European imagination has been classically analyzed in such studies as William J. Bouwsma’s *Venice and the Defense of Republican Liberty: Renaissance Values in the Age of the Counter Reformation* (1968) and Tony Tanner’s *Venice Desired: Writing the City* (1992). However, Bouwsma and Tanner traced the myth when it mattered most to political thought (mainly before the mid-seventeenth century) and to literature (mostly since the early nineteenth century). The aim of John Eglin’s brief, elegant, and thoughtful book is to reconstruct the various myths and counter-myths of Venice in British politics, art, architecture, and literature in the intervening period of transition from theoretical prominence to practical obsolescence. If the pickings are relatively slim, and Eglin’s conclusions somewhat inconclusive, that is less his fault than his subject’s. No longer the object of political fascination like James Harrington’s, but not yet the aesthetic

obsession of Lord Byron or Ruskin, *settecento* Venice presented no clear or consistent image to Britons. For almost two hundred and fifty years (until 1793), Britain and Venice maintained no diplomatic relations, and even at the height of the Grand Tour there were generally only thirty or so prominent British visitors to the city each year. Such tenuous contacts did not allow Venice to be entirely forgotten, but neither were the remaining myths and memories insignificant of broader political and cultural developments in Britain, that other burgeoning maritime power so proud of its own constitutional balance and commitment to liberty.

Eglin identifies three components of the myth—Venice as the much admired *stato misto*, as the ambivalent *stato di libertà* (both political and moral), and as the decadent *città galante*—which nourished three overlapping British attitudes: the positive myth of Venice, a sceptical counter-myth, and a more general “Venetotropism” that intermittently drew attention to Venetian parallels with British culture. Eglin admits that the typology of myth and counter-myth breaks down early in the eighteenth century, but “Venetotropism” is a less than handy term for the rather formless phenomenon of the later period. He persuasively shows that attachment to the various myths of Venice generally failed to map precisely onto the fluid political allegiances of Whig and Tory. The prominence of Venice as a political metaphor declined after 1700 as it was “used less, and less explicitly, as a mirror that reflected glory on its fellow maritime, commercial, and increasingly imperial quasi-republic” (p. 34); instead, comparisons with ancient Rome predominated, especially after the Seven Years’ War. Thereafter, British travellers also seem to have become less prey to the earlier myths and more willing to rely on their own experiences, even as they imported Venetian paintings (and painters, notably Canaletto) and sought to imitate Venetian civility, whether in their Palladian villas or at the Society of Dilettanti.

Eglin’s account of the Venetian myth in its decades of decline is scrupulous but inevitably scattershot, as Palladianism, the masquerade, and the Dilettanti fall under the rubric of “Whiggish cultural politics,” or patricians, prostitutes, and priests become the prime subjects of tourists’ voyeurism. His approach is synthetic rather than analytic, save in the fine chapter on the eighteenth-century reception of Thomas Otway’s Venetian conspiracy drama, *Venice Preserved, or A Plot Discovered* (1682). Though admirably executed, and true to its proposed aim, this book ultimately shares the weaknesses of many studies which trace only representations. It offers only one side of the conversation, as no attempt is made to recover Venetians’ images of Britain (or their response to Britons’ images of them). It neglects potentially illuminating comparisons—with eighteenth-century British conceptions of Naples, the Dutch Republic, Poland, or Sweden, for example, or with other European cultures’ images of Venice. Later cultural manifestations of British identification with Venice, such as the palazzoesque fa-

acades of nineteenth-century Glaswegian warehouses, remain unexplained. This book may not noticeably change the landscape of eighteenth-century British history but, like William Marlow's marvellous capriccio of ca. 1795 portraying St. Paul's Cathedral beside a Venetian canal, it does entertainingly place familiar landmarks in an illuminatingly unfamiliar setting.

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EDWARD ROYLE. *Revolutionary Britannia? Reflections on the Threat of Revolution in Britain, 1789–1848*. New York: Manchester University Press. 2000. Pp. ix, 214.

How did Britain avoid revolution when so many European governments were shaken in the stormy period between 1789 and 1850? Until the publication of E. P. Thompson's *The Making of the English Working Class* (1963), the orthodox view was that in spite of intense economic distress, the threat of revolution and actual uprisings resulted from the activities of marginalized minorities. Historians dismissed the very real concerns of the authorities as misplaced, and the waves of repression as an overreaction to the exaggerated reports of police spies. By contrast, Thompson insisted on the centrality of a radical politics within the various protest movements and on continuity, in the form of a diffuse and subversive current of ideas, between the Jacobin popular societies of the 1790s and emerging Chartism in the 1830s. Nevertheless, this was not taken to imply that there had been a significant revolutionary threat. Edward Royle goes a step further, arguing that the potential for revolution did exist in the form of active minorities, committed to violent action. For this potential to be realized depended, on the one hand, on such factors as the quality of leadership, effective organization, and wider support, and, on the other, on the existence of divisions within the social elite and incompetent government crisis management. These were missing ingredients.

Assessing the seriousness of the menace of subversion, then and now, depends on the weight given to the government's sources of information. Royle tends to accept contemporary official judgments that much of the intelligence provided to the government was of a high quality. This information suggests that from late 1792, widespread misery enlarged the support for political reform and, as it was not forthcoming, for revolution. The pro-French discourse of the popular societies; reports of arming in London, the north, and Scotland; the activities of the United Irishmen; the unrest caused by subsistence crisis in 1795; and the prospect of invasion and the naval mutiny in 1797 represented threats sufficient to explain widespread social fear and counterrevolutionary repression, which had the effect of forcing radical activists underground. The revival of agitation during periods of economic distress like 1811–1812 or as a result of the postwar crisis, and renewed reports of plotting and arming, again sounded the alarm. Tension was especially high

in London, the textile districts of the north, and the Glasgow area in the winter of 1819–1820. Once more, the official response was primarily repressive. The early 1830s brought further unrest stimulated both by economic depression and another French revolution. An extremist political discourse in the press and mass meetings condemned the rule of a "vile" and "corrupt" aristocracy. Serious disorders in London, Bristol, Derby, and Nottingham and insurrection in Merthyr Tydfil, a major center of the iron industry in an area with a tradition of violent labor protest, together with the rising of Lyon silk weavers in the same year, had a substantial impact on public opinion. In this situation, the 1832 Reform Act was a means of at least partly satisfying the demands for representation made by the middle classes and detaching them from an increasingly frightening current of mass protest. It had the further virtue of not significantly altering the social balance of power.

Initially, Chartism represented nonviolent protest on the part of those excluded from this new settlement. Increasingly, however, local social grievances were linked to national political issues. Critics of moderation, especially in the industrial districts, employed an ambiguous discourse, justifying physical force as a last resort. The development of a mass movement, another rising in South Wales in 1839, and arming in parts of the north frightened moderates, who in 1840 through the National Charter Association reiterated their commitment to "moral force." During the following decade, however, the involvement of Chartists in strikes and rioting represented a clear challenge to the established order. The European revolutions in 1848 provided further encouragement, stimulating probably the most serious revolutionary conspiracy since the 1790s, a development that, unfortunately, and in spite of the pioneering efforts of John Saville, Royle fails to address adequately. Given the central role of capital cities during the "year of revolutions," he might also have considered more fully the situation in London. There, as elsewhere, the potential for revolution was considerably weakened by personal and social differences and by political disagreements among radicals. Most of them, in demanding respect for the supposed "historic" liberties of "the freeborn Englishman" and condemning "arbitrary" government, would probably have preferred peaceful, constitutional reform. However, in times of economic distress, or in reaction to heavy-handed government, a revolutionary situation might have developed, almost by accident, as in France. The conduct of government was, in this respect, of crucial importance, and a more developed comparative perspective would surely have been instructive.

No doubt correctly, Royle insists on the strength of the established order. A small, relatively united governing elite engaged in an increasingly judicious mixture of repression and concession. Although repression had been given priority in response to the dire situation of the 1790s, the 1832 Reform Act proved

that peaceful change was possible. This was the product of an enlightened, aristocratic central government making use of an effective system of local administration, and occasionally of repressive force, while remaining committed to the rule of law; the latter a key factor in enhancing both the legitimacy of the executive and the “innate constitutionalism of British radical protest” (p. 170). Considerable efforts were also made by the ruling classes to encourage conservative and loyalist opinion, as well as popular xenophobia. Religion, education, and the media were employed with growing effectiveness to reinforce intimidation. The essential objective was to preserve “the people,” perceived as essentially “good hearted” and “deferential,” from the influence of a “perverted” minority.

In what became a commonplace of Victorian liberal ideology, the Glorious Revolution of 1688 and the 1832 Reform Act were contrasted with the chaos of revolution on the continent in 1848, appearing to confirm the superiority of the British way. Revolutions were for foreigners. Memories of the threat of revolution on John Bull’s island were largely erased. Royle has provided a valuable corrective, explaining why a revolutionary situation failed to develop but taking the threat far more seriously than most of his predecessors. Indeed, as in continental Europe, the menace of revolution would prove to be a major stimulus to the “modernization” of the British state.

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JAMES A. SECORD. *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation*. Chicago: University of Chicago Press. 2001. Pp. xix, 624. \$35.00.

An 1847 comic article in *Punch* called “The Book That Goes A-begging” describes a tome that “ran about town for a long time, knocking at every scientific man’s door; but the answer invariably was, ‘No thank you, my good book; we don’t want anything in your way.’ . . . It was always denied, and threatened with the police ‘if it did not carry its rubbish elsewhere.’” The author of this satire, Horace Mayhew, added that: “The *Vicissitudes of the Vestiges of Creation* would make quite a pathetic little book.” James A. Secord has written anything but “a pathetic little book” about the anonymously authored 1844 bestseller, *Vestiges of the Natural History of Creation*. Both for *Vestiges* and for Secord’s study of it, fascinating and encyclopedic would be closer to the mark than pathetic and little.

In the course of his thorough reconstruction of the publication and reception of the most popular forerunner of Charles Darwin’s *Origin of Species* (1859), Secord has much to say about early Victorian reading, authorship, book publishing, and reviewing, as well as about science, scientific institutions, and the popularization of scientific ideas. His focus on a single book, written by Scottish publisher and disseminator of

“useful knowledge” Robert Chambers, pays off in the form of an extraordinarily rich cultural history of early Victorian Britain.

Vestiges was read and discussed by all sorts and conditions of readers, from Queen Victoria to Chartist and Owenite radicals and atheists. As Secord notes, “It was unprecedented for a book of science to attract so much attention” (p. 37). Chambers’s daring speculations about “development” or evolution dealt with everything from “the nebular hypothesis” to gnats and earthworms. Its popularizing rhetoric, colorful examples, and narrative elements made it, as Secord says, a sort of “cosmic *Waverley*.” And Chambers’s initial anonymity, like Sir Walter Scott’s, helped make *Vestiges* an even greater sensation. Indeed, Secord has much to say about anonymity as an authorial and publishing tactic—in Chambers’s case, of course, adopted primarily because of the controversiality, both religious and scientific, of the theories he espoused. Well before Darwin, Chambers had his readers worried about whether they were descended from the apes, as in Elizabeth Barrett’s half-joking response. *Vestiges*, she declared, made her determined “not to be a fully developed [sic] monkey if I could help it” (Secord, p. 167).

Secord has searched, it seems, every nook and cranny of Victorian culture between 1844 and the 1860s. His thoroughness as a cultural historian extends down to the smallest details of the production and marketing of *Vestiges* including the techniques used for printing and binding the first edition. At times, Secord’s thoroughness leads to the illumination of fascinating aspects of early Victorian culture, including what he calls “Grub Street science” and the exhibition of various scientific and pseudoscientific marvels—everything from Charles Babbage’s calculating engine to Jenny the talking orangutan (p. 441). His treatment of the Chambers brothers’ publishing endeavors and their connections to George Combe and phrenology are equally fascinating. And his many, detailed accounts of how individual readers responded to *Vestiges* add an important chapter to the history of Victorian publishing and reading.

Secord’s thoroughness is mainly a virtue, but it is also a weakness. His book is often repetitious. Thus, Secord cites Mayhew’s *Punch* lampoon early in his study (p. 22), complete with one of the *Punch* illustrations and lengthy quotations from it. He cites it again, with a brief summary (p. 457); he reproduces the entire lampoon as an illustration (p. 459); and he quotes it yet again (p. 460). Besides the question of repetition, there are the questions of proportion and relevance: just how significant to the reception of *Vestiges* and Chambers’s ideas about “development” are the details about the printing and binding of the first edition? Could not those details have been relegated to a footnote? This book could easily have been 100 pages shorter without losing any of its substantive points. But these complaints are minor. Secord has produced a superb and wonderfully well-illustrated cultural his-

tory, one that should be read by everyone interested in Victorian Britain and in the history of scientific ideas leading up to Darwin.

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K. D. M. SNELL and PAUL S. ELL. *Rival Jerusalem: The Geography of Victorian Religion*. New York: Cambridge University Press. 2000. Pp. xvi, 499. \$74.95.

This monumental work grew out of a long study of the cultural regions of Britain, and concern for regional characteristics runs through this historical geography of religion. It is an appropriate concern for K. D. M. Snell and his research associate, Paul S. Ell. But the reader will be struck most forcibly not by the book's geographical emphasis but by its essentially quantitative character. It is keyed to a computerized analysis of the 1851 religious census, that most abundant (if problematical) source of statistics. The authors hope that "the relatively impartial quantitative methods adopted here" (p. 10) will yield more certain results than the qualitative approaches more usual in religious historiography, which they regard as a methodologically backward field, and that they will effect a transformation of religious history (and other social sciences).

The first part of the book is an analysis of the 1851 census, a unique source, whose registration-district data are computerized and mapped. The authors recognize problems with the census, particularly its totals of attendances and the adjustments for "twicers" and "thricers." They conclude that the census was carefully conducted, and that its general reliability is attested by the correlation of several indexes drawn from it. The "twicers" and "thricers" are neatly disposed of by an index of attendances as a percentage of total attendances, which gives the relative strength of the various denominations. This is a technical achievement that turns a census in which no absolute figure can be relied on into a reliable barometer of relative strength.

But is it a measure of total "religiosity" (crudely defined as attendance)? The authors too readily agree with Horace Mann (who organized the census) in his refusal to take a census of religious profession, which would have materially and perhaps unjustifiably increased the Anglican figures. Statistics (not used here) of baptisms and marriages indicate much occasional use of church services by infrequent attenders, making it at least the residual Church of England and giving it claim to be the national church. The refusal to adjust for occasional use (hard though that is to quantify) gives apparent historical sanction to a Victorian stereotype: the insistence that religion be active and frequent, a lifestyle unacceptable to the working class, which was thus left out. The great unstated fact about the Church of England is its rejection of nationality in favor of individual activity, denominationalizing itself to compete with denominationalism.

Chapters deal specifically with Anglicans, Roman

Catholics, and "old dissent" and "new dissent" in England and Wales (the Scottish data proved unusable). This is followed by a study of the spatial relations of these groups, which shows "the striking southern and midland geography of the Church of England and of 'old dissent,' and the more northern and south-western geography of the Methodist denominations" (p. 13). Snell and Ell thus seem to settle an old historiographical debate about the spatial complementarity of "old" and "new dissent." Also established is the distinctiveness of Wales, ethnicity predominating over the socioeconomic variables discussed for England. Political implications are suggested but not developed.

The second part of the book, examining socioeconomic variables, required the construction of data sets at the parish rather than registration-district level for fifteen diversely representative counties. Suitable parishes could be compared with the Compton Census of 1676, searching for continuities for "Papists" and Dissenters (weak for Dissenters, somewhat stronger for Catholics). Another chapter examines the important Sunday School movement, finding Anglicans doing surprisingly well, Wales once again a special case (especially the Calvinistic Methodists), and a pattern of control correlating with the amount of child labor. Next the question of free or appropriated sittings is studied, Anglicans generally getting high marks for openness but producing resentment when they were not. The relationship of landownership to open or closed parishes is traced, using an infrequently consulted source, the *Imperial Gazetteer* of the 1870s. Finally, the question of urbanization and its relation to "secularization" is discussed, with a view to a projected extension of this study in a second book by Alasdair Crockett (another research associate). The tentative conclusion is that "secularization" (defined here as non-attendance) is a product of religious pluralism, which created "an environment of relative freedom of choice" (p. 304), especially in border regions and places of extreme urbanization. This will be an exciting and contestable book.

The authors' proposal to transform the field of religious history may be perceived as a challenge to its nonquantitative practitioners, who prefer that evidence should be weighed rather than counted. Even recent social science-oriented scholars have treated religion and its institutions in terms of lived experience rather than statistics. The new approaches supplement but do not supersede the old. What the authors have done is to open up new dimensions of religious history, both regional and quantitative. In this sense, their work is indeed pathbreaking and stands as a landmark in social science.

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R. J. M. BLACKETT. *Divided Hearts: Britain and the American Civil War*. Baton Rouge: Louisiana State

University Press. 2001. Pp. xiii, 273. Cloth \$49.95, paper \$29.95.

For most historians of the American Civil War, relations between Great Britain and the United States only appear occasionally in the grand narrative. Students learn that the Tariff Act of 1861 antagonized British shipping interests, and that the Federal arrest of Confederate agents James Mason and John Slidell off the British mail packet *Trent* threatened to drive England into the arms of the Confederacy. Perhaps England figures most prominently when Civil War historians reconstruct the variables weighed by Robert E. Lee and by Abraham Lincoln in the fall of 1862, when Lee opted to invade the North and Lincoln decided to issue the Emancipation Proclamation. Both men, so the story goes, believed that what they did might affect the ongoing debate in England over the Civil War.

This book shifts the focus across the Atlantic, examining the contours of the British debate over the American Civil War. R. J. M. Blackett's focus is not as much on the British government's actual policy alternatives as it is on wartime political discourse about the American conflict. Taking Mary Ellison's classic study, *Support for Secession: Lancashire and the American Civil War* (1972), as a point of departure, Blackett has assembled a wealth of information about the dozens of clubs and organizations, and the hundreds of participants, in the nationwide British debate on the Civil War. Whereas Ellison's regional study found strong support for the Confederacy in the cotton textile country, driven largely by economic self-interest, Blackett's comprehensive research reveals a much more finely nuanced portrait of political allegiances and public discourse.

As the Civil War began, several factors shaped the British response, including a strong commitment to maintaining a powerful international voice, an enduring pride in the nation's abolitionist history, and a spirited debate over political reform at home. These factors combined with an almost familial interest in American affairs to ensure that the Civil War attracted substantial British attention. Hundreds of men and women joined organizations supporting either the Union or the Confederacy, and thousands attended public lectures and meetings or read over editorials, letters to the editor, and political pamphlets about the American conflict.

Blackett has mined the records of dozens of organizations to construct a careful demographic analysis of 867 identified club members, providing a new portrait of Union and Confederate supporters. Although the Confederacy certainly had substantial support, particularly in the textile regions and on the pages of the London *Times*, the pro-Union forces generally outnumbered and outcampaigned their adversaries, even in Lancashire and Cheshire. No single variable fully explained membership in the competing camps, but several patterns do emerge. Except for those few who

remained neutral out of dissatisfaction with the Lincoln administration, most abolitionists supported the Union. And although abolitionism seemed to have lost much of its power by the late 1850s, the politics of slavery reemerged with the issuance of the Emancipation Proclamation. When religious leaders entered the debate, Anglicans tended to side with the Confederacy whereas Dissenters lined up in favor of the Union. And across the political spectrum, British observers saw the Civil War as a test of American democracy in action, with reformers joining pro-Union clubs while conservative leaders and members of the aristocracy commonly found political and cultural kindred spirits in the South.

Such broad conclusions can be teased out of this detailed monograph, but not without substantial effort. As Blackett explains, "the complexity and subtlety of reactions to the war are almost staggering" (p. 15). At times he appears more intent on illustrating the staggering complexity than in illuminating the larger patterns. Thus, the observation that nearly all Dissenting ministers in the sample backed the Union is followed by a meticulous discussion of the two exceptions to that rule. The accumulated weight of dozens and dozens of capsule biographies of local leaders sometimes leaves the reader wishing for more attention to the forest and less to the trees. It is as if Blackett were writing too close to his copious notes, not only sharing seemingly unnecessary biographical details, but, on several occasions, pointing out where "nothing is known" about a particular individual. And where the details are more clearly linked to the analysis, the book's architecture produces frustrating redundancy. In the second chapter, we learn how six leading abolitionists came together in May 1859 to form the London Emancipation Society; twenty-two pages later we discover the identical information, down to the names of those six founding members. The occupational distribution of the seventeen members of the Sheffield branch of the Southern Independence Association is certainly worth examining, but it is not clear why the same information must appear in consecutive chapters.

This book brings to light the complex British debate about the American Civil War, but in a form that will likely appeal only to a limited readership.

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ROBERT F. HAGGARD. *The Persistence of Victorian Liberalism: The Politics of Social Reform in Britain, 1879-1900*. (Contributions to the Study of World History, number 77.) Westport, Conn.: Greenwood. 2001. Pp. 209. \$62.50.

Robert F. Haggard's fine parliamentary history examining the philosophic origins of, and justifications for, British institutionalized social reform seeks to do two things. First, it attempts to find a "fundamental ideological break" between Victorian liberalism and the

twentieth-century welfare state. Second, it gives renewed scrutiny to that uniquely Victorian mix of liberalism, socialism, and evangelicalism that contributed to political and social reform at the end of the nineteenth century. Haggard's careful chronological examination of the late Victorian period focuses on key economic events and trends, as well as on public opinion and parliamentary and extraparliamentary organizations. He methodically reexamines the philosophical justifications for social disparity as well as the more pragmatic creation of programs designed to minimize it.

Haggard's book begins with an excellent historiographical introduction. Readers have few questions as to the standard debates and interpretations concerning the Victorian laissez-faire approach to poverty and social reform and that of the collectivists. He then reassesses those debates through several carefully constructed chapters addressing key economic and social challenges in the Victorian era. Haggard's chapter on the "Great Depression" of 1873–1896, for example, contends that in spite of challenges from new industrial nations, a declining population, and fears about security throughout the vast empire, Britain was more inclusive and responsive to the public's social concerns than ever before. An expanded electorate, improved technological education, and changes to domestic manufacturing were problematic not for the working class in Britain, but rather for the middle class. Ironically, he claims, it was the middle class that revived the "Condition of England" question in the 1880s.

Haggard next revisits the studies of Henry Mayhew, Charles Booth, and B. S. Rowntree, as well as the popular political campaign to educate the British public as to the moral and material problems facing the urban and industrial poor. While he does not dismiss them out of hand, he does question the real impact the "Condition of England" debates had in exacting any real social or conceptual change in Britain. That assistance to the poor continued in much the same way after these studies as before is born out in his next chapter on philanthropy and poor relief. Here, Haggard demonstrates that the newly created Charity Organization Society effectively curtailed indiscriminate poor relief offered by organizations such as the Salvation Army on the basis that this resulted in the demoralization of the poor. Since poverty was, at least in part, the result of moral failings, the solutions, this and similar Victorian organizations argued, should come from private and not political sources.

Haggard's argument concerning socialists and organized labor is perhaps the most compelling part of his work. Claiming that "all the sound and fury of the late-nineteenth century socialist movement had not amounted to very much" (p. 115), he convincingly shows that theorists and organizations were largely isolated and marginalized as Victorian politics goes. The Conservative, Liberal, Radical, and Progressive parties all proposed ideas as to how the state could improve the lives of everyday people, and that was

demonstrated by a flurry of legislation passed by both the Conservative and Liberal governments of Benjamin Disraeli and William Ewart Gladstone. Joseph Chamberlain's experiment in Birmingham and the Progressive Party's proposed "betterment" taxes all displayed the now-universal recognition that the state did have some social responsibilities. But as Haggard shows in his chapter titled "Limits of Governmental Intervention," most people rejected the socialists' message in favor of a more limited role for the government and organized societies like the Liberty and Property Defense League and the London Ratepayers League to agitate on behalf of those positions. The majority of Victorians, he argues, still believed "that government programs and regulations tended to limit individual freedom, demean character, reduced industrial competitiveness, decrease wealth, and increase pauperism" (p. 174). By 1900, Haggard concludes, British conceptions of social reform had not strayed very far from the classical liberal path of the previous century.

This book provides an excellent reexamination of middle and upper-class views of social reform from 1870 to 1900. Indeed, Haggard argues not only that the late Victorians never abandoned their principles of "self-help, character, private charity and local initiative" (p. 175), even as the late-century economy weakened, but also that traditional historical arguments that the formation of the Labour Party cured economic ills are weak in comparison to the more compelling evidence that organized socialism served more successfully to rally traditional liberal policies and politicians in opposition to state expansion. Haggard shows that later Victorians only differed as to the degree of state intervention. His conclusions throughout the work remain constant: nothing in the late Victorian era paved the way—ideologically, organizationally, or culturally—for the eventually establishment of the British welfare state. That required World War I and a completely new set of economic and social problems for society and governments to address.

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CHRISTOPHER BREWARD. *The Hidden Consumer: Masculinities, Fashion, and City Life 1860–1914*. (Studies in Design and Material Culture.) New York: Manchester University Press. 1999. Pp. ix, 278. Cloth \$79.95, paper \$24.95.

Until comparatively recently, no mainstream historical journal would have reviewed a work concerned with fashion. Two historiographical developments explain why the subject has stepped outside its specialist ghetto and transcended a longstanding association with trivia. First, the recent scholarly interest in consumption has seized on dress as one of the most visible indicators of consumer preference and consumer behavior. Second, the new cultural history, with its focus on the everyday and mundane, finds in clothing one of

the most revealing signifiers of popular culture, in which the manufacturer, the wearer, and the spectator account for a rich diversity of meanings.

Christopher Breward's study of men's fashions in Victorian and Edwardian Britain extends both these perspectives in innovative and important ways. Firstly, he takes seriously the need to understand the production side. This does not extend to the sweated trades, which made up the finished cloth, but Breward offers a path-breaking analysis of the organization of London tailoring and retailing. Too much writing on fashion history has focused exclusively on the wealthy. One of the most refreshing features of this book is its comprehensive social reach; the economic base of men's fashion is vividly depicted, from the high-class West End establishment to the "misfit dealer" of the East End. Indeed, the book comes close to providing a complete sartorial topography of central and east London. On the strength of his research in trade journals and catalogues, Breward is able confidently to advance the argument that men's clothing was a no less fertile ground for urban entrepreneurship than was women's clothing.

Most readers, however, will turn to this book as a contribution to the cultural history of dress, and it is here that its real importance lies. Breward places men's fashions firmly in the field of gender history. He thus does much to redress two defects in recent scholarship: the failure of fashion historians to deal adequately with men's dress, and the peripheral position of dress in scholarly writing about Victorian masculinity. Breward by contrast asserts that fashion and body language were as central to masculine identity between 1860 and 1914 as they have been in recent decades. This claim is one that historians of post-1945 fashion might regard as overstated, but it captures the vigor and variety of men's sartorial culture on the eve of World War I. The common assumption has been that, while ample allowance was made for the vagaries of feminine taste, men who had the means to be discriminating in their purchase of clothes could exploit only a trifling degree of choice; fundamentally they dressed alike, and on very much the same lines from one season to the next. Breward shows that this picture became steadily less accurate from mid-century onward. Here, too, the social range of the book is impressively comprehensive. Breward draws on his knowledge of popular novels and autobiographies to lay bare a highly sophisticated metropolitan dress culture, and he analyzes music hall with particular effect to bring out the importance of envy, emulation, and pastiche that played on every gradation of sartorial status. There are few surprises in the account of the gentleman's wardrobe, with its change of outfit for every indoor and outdoor pursuit. Less familiar is the image of the East End coster purchasing two "best" suits a year: cheap and shoddy, but up-to-the-minute and suitable for display on the Saturday evening "monkey parade."

But gender is as important to the analysis as class.

Breward takes the scholarly literature on manliness as his point of departure and quickly shows up its limitations. Contemporary homilies emphasized the moralistic and ascetic qualities of manliness, and modern interpretations routinely define it in these terms. But the leisured urban milieu described here seems a world away. The sartorial pace was set by the bachelor, behaving less as an aspirant to full manhood than as a consumer for whom clothing was no less integral to his lifestyle than drinking and womanizing. Breward attributes the contradiction to the enduring appeal of a dandified approach to clothing that had persisted since the 1830s and was increasingly elaborated toward the end of the century (Oscar Wilde is briefly—but illuminatingly—positioned in this larger sartorial context). But the disparity also arises from a lopsided reading of manliness. The authors on whom Breward draws—notably James Eli Adams—are almost exclusively concerned with the elite discourse of manliness. What have still to be reconstructed are the manly values acknowledged by the shopkeepers, clerks, and costers who constituted the bulk of Breward's consumers: they were certainly more hedonistic and less self-controlled than our current accounts of manliness would suggest. There is, then, more to be said about gender and men's clothing in this period, but this book is by far the best contribution to the subject thus far.

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STEPHEN HEATHORN. *For Home, Country, and Race: Constructing Gender, Class, and Englishness in the Elementary School, 1880–1914*. (Studies in Gender and History, number 15.) Buffalo, N.Y.: University of Toronto Press. 2000. Pp. xii, 300. \$50.00.

Stephen Heathorn takes what seems a straightforward question and shows that it is more complex than it seems. He asks how it was that England's elementary schools helped construct a sense of national identity among working-class children in the years from 1880 to 1914. In the heyday of English power, this might have seemed an easy task. Working-class children, although at the bottom of their own country's social order, could take pride in the British Empire. Yet Heathorn argues that the authors of England's primary school readers in history, geography, and English literature faced dilemmas in trying to build a sense of national identity. Most importantly, they had to create a master narrative that appealed to working-class children and stirred their emotions, even while reinforcing class, gender, and racial hierarchies. Increasingly, England's school texts presented a personality-driven history, replete with good kings and bad kings and a romanticized and racialized vision of the Anglo-Saxon heritage.

Heathorn does not analyze secondary texts but rather school readers designed for primary school students. These, he points out, sold in far larger numbers than did formal texts, truly reaching a mass audience. Between 1885 and 1902, Longman published

480,000 copies of its *New Reading Books*, while formal history books had runs of less than 35,000 copies. Heathorn also notes that upper-level texts primarily reached middle and upper-class students, but primary readers reached England's masses. Finally, schools bought readers for individual students, while upper-level texts were often intended only for school libraries.

Heathorn focuses on how a master narrative was developed and to a lesser extent, on how it changed from the 1880s to the first decades of the twentieth century. Victorian text authors wanted to avoid lists of battles and monarchs that would bore children rather than inspire patriotic sentiment. They tried to appeal to working-class children by creating simple narratives, extolling both heroes such as Lord Nelson and the common people whose hard work and reliability guaranteed English triumphs. They emphasized England's Anglo-Saxon heritage, with its people held to be fair, well-formed, and valiant. This created a problem when discussing the Norman Conquest, but the authors held that the Normans, although racially distinct from the Anglo-Saxons, soon amalgamated with them to fight common enemies.

The authors tried to humanize their accounts by presenting the English government as a larger version of the ordinary English home. Just as a constitutional monarch governed within constraints, so the father governed the domestic hearth within limits. Girls were exhorted by story and prescription to become models of domestic efficiency, contributing in their modest but indispensable way to England's betterment. This domestic prescription was clearly inadequate to accommodate the figure of Elizabeth I. She was far too important and dramatic a figure to pass over lightly, yet she fit uneasily into the master narrative and its narrow view of gender roles. The readers resolved the dilemma by glorifying Elizabeth the queen while denigrating Elizabeth the woman. Although a great ruler, she was not, the readers said, a pleasant or a good person. They also returned the focus to men of action by dwelling upon the exploits of the Elizabethan "sea dogs."

Heathorn argues that, as the nineteenth century drew to a close, the readers became increasingly conservative. They laid more stress on the monarch as the great symbol of the nation. They almost abandoned the tradition of upholding the struggle for freedom as the core of English national identity, an outlook found in some readers from the early 1880s. Heathorn cites a reader from 1882 that declared: "All Englishmen are now agreed that we greatly owe the freedom we now enjoy to our forefathers, who resolved to bleed and die on the battlefield rather than submit to the arbitrary will of misguided kings" (p. 106). Such stirring sentiments diminished as racialized explanations of the English national character became more prevalent. This served to include working-class children while not raising difficult political issues. It also justified England's rule over many of the world's peoples. The

readers tried to engage children's interest by highlighting the physical and cultural oddities of diverse peoples, from the authors' perspective; those peoples were ranked in virtue from the Maori, presented as noble fighters, to the Australian Aborigines, presented as barely human. Heathorn attributes these changes both to internal developments in the book industry and to the influence of a conservative Darwinism. In his view, even English social reformers increasingly framed their goals in nationalist terms, although a minority criticized the jingoism of school readers.

Heathorn provides a welcome and carefully researched account of England's school books and how they tried to make working-class children feel a strong sense of national identity. His analysis offers much insight into how patriotism increasingly became something that was taught rather than part of an informal cultural heritage. It would be revealing to undertake a similar analysis of primary readers in other countries, to assess the terms in which their educators tried to instill national sentiment in their working-class children.

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JENNY HAZELGROVE. *Spiritualism and British Society between the Wars*. (Studies in Popular Culture.) New York: Manchester University Press. 2000. Pp. 294. Cloth \$74.95, paper \$29.95.

In this book, Jenny Hazelgrove offers a lively and persuasive analysis of Spiritualism and its cultural context in interwar Britain. While most historical treatments of Spiritualism, mediums, and parapsychology focus on the nineteenth century, Hazelgrove argues that the 1930s was the highwater mark of British Spiritualist activity, a "boom time" supported by bereavement, traditional views of femininity, and superstition. Hazelgrove's example from her own life—her atheist father who nonetheless keeps fresh flowers arranged by the photograph of her dead brother—underscores how rituals for the dead function more for the living. Even someone who does not believe in contact with the spirit world, or an afterlife, is comforted by ritual communion with a deceased loved soul.

Drawing on the work of historian Jay Winter on the psychological and spiritual aftermath of the World War I, Hazelgrove shows how Spiritualism offered a personalized, concrete consolation and a ritual in which women could play significant roles. It incorporated religious beliefs and practices and borrowed specific mythologies and rituals from Catholicism: the Madonna/medium interceding on behalf of the dead, the mass-like séance, the guardian angel as spirit guide. Hazelgrove first examines the institutional and pseudoscientific structure of the movement, and its claims to make contact with the dead through mediums and the séance. There were several branches of

Spiritualism, each with its own priorities and style. Those inclined to the scientific method aligned themselves with the British Society for Psychical Research (SPR), which had been founded in the 1880s. Their rivals belonged to the more flamboyant National Laboratory of Psychical Research (NLPR), headed by the aggressive, attention-seeking Harry Price. More than a hundred people, most of them women, claiming supernatural powers passed through his laboratory between 1923 and 1937. Price treated his female experimental subjects differentially according to their social class and beauty, courting and flattering the well-educated, attractive medium Stella Cranshawe while bullying and tormenting Helen Duncan, a working-class mother of nine. In his writings, he compares Cranshawe to a flower and Duncan to a cow. The split mirrored the way Price, and others at the time, saw mediums as representatives of ideal or predatory femininity, to be cherished or controlled. A third, privately controlled organization was the British College of Psychic Science, founded by Spiritualist converts Barbara and Hewer McKenzie in 1920. This organization also exploited and controlled young women mediums, demanding their submission or else exposing them as frauds (which meant arrest and prosecution under the Vagrancy Act of 1824).

The most significant factor in the Spiritualist movement was gender. The female medium served many purposes in an ongoing dispute over femininity, as Hazelgrove shows in her analysis of the ways in which women became mediums. Following the work of Alex Owens on Victorian mediums, she shows that female mediums both benefited from the movement's "privileging of feminine insight" and were marginalized in the society as a whole. The endurance of a traditional conception of woman as nurturing, passive, and reproductive supported metaphors of the seance as a birth process, in which the medium figured as archetypal mother. But the medium's trance could also be interpreted as a sinister form of sexual possession, sometimes with masochistic elements of being dominated and victimized. Mediums were often seen as "sexual deviants" or the bearers of some strange and mad sickness"; at the same time, the eroticization of female sexual submission in popular culture, and bestsellers like *The Sheik* (1919), made the persona of the possessed medium covertly seductive even when denounced as evil. Spiritualism benefited from collective fantasy about female ghosts, spirit-guides, and phantoms, as illustrated in the novels, films, and plays of the era, from Thomas Mann's *The Magic Mountain* (1924) to Noel Coward's "Blithe Spirit" (1941).

Hazelgrove concludes that the decline in Spiritualism is related to the decline in traditional assumptions about gender and the rise of feminism. But her own evidence also suggests that the forms of Spiritualist superstition have changed rather than disappeared. Now the humble medium is more likely to aspire to the status of a priestess or goddess, while the female channelers on the psychic networks continue to profit

from the grief of the needy and gullible. This fine study is both a worthy addition to the literature of the field and a warning about how emotions may lead to a resurgence of Spiritualist belief in troubled and anxious times.

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SUKE WOLTON. *Lord Hailey, the Colonial Office and the Politics of Race and Empire in the Second World War: The Loss of White Prestige*. (St. Anthony's Series.) New York: St. Martin's. 2000. Pp. xii, 221.

Sir Malcom Hailey, first baron Hailey of Shahpur and Newport Pagnell, lived a long life during an important transition period in the history of the British Empire. Born in 1872 at the height of British influence in world affairs, Hailey spent most of his adult life in the service of his sovereign as a member of the Indian Civil Service, a fact reflected in his peerage. He then lived to see the empire disintegrate before his death in 1969. Suke Wolton sees Hailey as a venue to study changing views of the empire: "Hailey provides a useful prism through which to examine the changing racial and imperial concerns because he was not a maverick, nor an outsider, but one of the old establishment, who, despite his background, changed his views about colonial administration during the Second World War" (p. 10).

As Wolton is quick to note, Hailey was always interested in preserving the empire. This British institution had long depended on the idea of white superiority to legitimize its rule of the colonies. Whatever legitimacy this idea had ended with Japanese victories over the European colonial powers and the United States in 1941 and early 1942. Asians had defeated some of the greatest white nations on the planet. The British still considered themselves superior to their colonial charges, but they began looking for other factors to use to explain their dominance. Americans were also challenging the empire as an undemocratic form of government. Hailey was a leading figure in the defense of this British institution to the "cousins" on the other side of the Atlantic. At a semi-official conference attempting to plan for the postwar world held in December of 1942, he argued that the empire served several functions. In doing so, Hailey also challenged the moral authority of the United States. The empire allowed the British to project power in international affairs and to provide for collective security, a gentle reminder to the Americans of their interwar isolationism. He reminded the Americans that they could hardly lecture the British on race, given the practices in the U.S. South and the segregation of the armed services. He also told Americans that they did not have a monopoly on humanitarianism: the empire had also helped improve the lives of dependent peoples living under the authority of the British government. The defenders of the colonial system found a solution to their problem: they began using rhetoric

that emphasized new missions for the British Empire. Issues such as minority protection, economic development, and social welfare replaced racial ideology. Such language appealed to the New Dealers in Washington. The British were also able to emphasize their role as the protector of their colonial subjects rather than white superiority.

This study is well written and easy to read. Walton has done solid research in the Hailey papers, the Public Record Office, the India Office Library, several holdings at Columbia University, and the U.S. National Archives. She also addresses an issue that is popular with scholars these days: race. This work, however, reads more as a study of intellectual propaganda in U.S.-U.K. relations than a study of race as a factor in the sovereign authority of the empire. Oddly, Hailey often disappears from these pages; Walton often seems to assert his influence rather than prove it.

Specialists writing on the British Empire will probably want to read Walton's book. They will, however, probably never have need to assign it to their graduate students. Generalists teaching broad surveys of British history can safely devote their attention to other works.

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DAVID DUNTHORN. *Britain and the Spanish Anti-Franco Opposition, 1940-1950*. New York: Palgrave. 2000. Pp. ix, 236. \$69.95.

In December 1937, Labour Party leader Clement Attlee visited Spain at the height of the Spanish Civil War. He met with members of the British Battalion of the XVth International Brigade who had come to Spain to fight for the Spanish Republic against General Francisco Franco and his insurgents. "We are proud," he told his fellow countrymen, "of the deeds of those who have died and those who still live." He denounced as a "farce" the non-intervention agreement, which ensured that Great Britain would not come to the aid of the Spanish Republic. With Labour's great victory in July 1945, it seemed inevitable to many that Attlee's government would lead the Allies in the removal of Franco. The Labour left insisted that Great Britain should set the world "a moral example" by doing so. Harold Laski roared, "At long last we are going to be in a position to do full justice to our Spanish comrades." But it was not to be.

David Dunthorn has provided us with a finely grained account explaining why the British adopted a policy of inaction toward Spain during and after World War II. Until the Allied landings at Normandy, Great Britain was eager to avoid confrontation with Franco for fear of driving him further into the arms of Adolf Hitler and Benito Mussolini. With the end of the war, the Foreign Office under Ernest Bevin offered rhetorical support for Franco's removal from power, calling his regime "an anachronism" but insisting that Great Britain would not interfere in the internal affairs of

another country, and that the matter of succession was for the Spanish people to decide.

Attlee and Bevin proved reluctant to intervene in Spanish affairs because Britain was faced with the prospect of political instability in postwar Europe. This meant that any threat of a new civil war in Spain was to be prevented at all costs. At first, looking at the individual claims of the Monarchists and Republicans, each of whom hoped to succeed Franco, the Foreign Office became convinced that neither enjoyed the support of the Spanish people. Therefore, if Franco's repressive regime were to be peacefully ousted, it was necessary to encourage a moderate Spanish anti-Franco opposition, consisting of both parties, to reconcile their ideological differences and interests, and, perhaps with more difficulty, their internal divisions. Once unified, this "third option" would present a viable and peaceful alternative to the dictatorship. If this occurred, Franco might be persuaded to give up power and consent to a transition to a liberal democracy, thus ensuring stability as well as the protection of Britain's commercial and strategic interests in Spain.

Dunthorn offers a masterful analysis of the reasons a moderate alliance never successfully emerged. He is particularly successful in delineating the contours of the complex and tangled world of the pretender, Don Juan, and the two exiled Spanish leaders, Indalecio Prieto and José María Gil-Robles, who sought to forge a rapprochement that might lead to a broad front against Franco. Scenario after scenario for unity was played out, sometimes offering real prospects of success, and then falling apart because of internecine doctrinal and personal rivalries as well as Franco's shrewd manipulation of the hopes of the pretender. The plotting, moves, and counter-moves of the opposition reached from Mexico to France to Portugal to Spain, possessing an increasing unreality as the exiled contenders for power became progressively isolated from Spanish opinion.

Strong anti-Franco rhetoric could not disguise the fact that the policy of Great Britain in the postwar years under Labour, as in the 1930s under the Conservatives, was to be one of non-intervention. The Foreign Office moved to the positions that Spain might be able to reform itself and, of underlying importance, that a stable Spain, even if Francoist, would be best for the country's interests. The advent of the Cold War proved decisive, for no European leader could be judged to be more anticommunist than Franco. So, Great Britain, leader of the war against fascism, bitterly disappointed the opposition inside and outside Spain that had expected the British to pave the way to the restoration of democracy.

As Dunthorn argues, the passivity of Attlee's government was understandable in light of the failure of the exiled opposition to become unified, Labour's understanding of Britain's national interests, and the advent of the Cold War. Nevertheless, his study presents an unusually vivid picture of the distance between the public declamation of principle and the

private and insistent assertion of a nation's strategic and economic interests. The Labour leader who once shouted republican slogans in Spain had now submitted his political enthusiasms to a postwar version of the once reviled non-intervention agreement.

The virtues of Dunthorn's study are many and incontestable. The clarity of his exposition and analysis and his exhaustive use of Spanish as well as British sources deserve high praise. Moreover, his may well be the last word that needs to be said on Great Britain's role in ensuring that the hopes of the British volunteers would not be realized for another thirty years.

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NIGEL TOWNSON. *The Crisis of Democracy in Spain: Centrist Politics under the Second Republic, 1931–1936*. Portland, Oreg.: Sussex Academic Press. 2000. Pp. xv, 444. \$65.00.

Nigel Townson's book is the best recent political history of the tumultuous Spanish Second Republic (1931–1936). Based on exhaustive research and nuanced arguments, his reappraisal of the maligned Radical Party is really a bold reinterpretation of the republic's political trajectory. At issue, as Townson says in the introduction, is whether the centrist forces, epitomized by the Radical Party, could have stabilized the republic, or whether Spain was polarized into irreconcilable camps from the outset. Although the question of whether the polarization that led to civil war resulted from long-term structural divisions or short-term political blunders has long been debated by Spanish historians, Townson provides a compelling new reading of post-1931 politics that highlights the impact of political choices on the republic's fate.

At the heart of Townson's rereading of republican politics is the Radical Party, universally reviled by contemporaries and historians as opportunistic and venal and by the Left as "reactionary." Behind this "black legend" of the Radicals, he argues, was an important centrist party with considerable popular support that played a key political role until the end of 1935. As part of the governing coalition in 1931, then as the main opposition party in 1932–1933, and finally as the fulcrum of a range of governments between 1933 and 1935, the Radicals have been unaccountably dismissed or minimized in most political histories of the republic. Instead, Townson argues that the Radicals had a defensible and consistent strategy of consolidating the republic through incorporation of the middle classes with moderate reforms. While their political choices did not always achieve the desired results, Townson identifies a basic ideological coherence that belies the accusations of opportunism or counterrevolution.

For the Left, at the time and since, the most controversial of the Radicals' decisions was their alliance with the non-republican Right, the Spanish Confederation of Autonomous Rightists (CEDA), during

and after the September 1933 elections. Defended by the Radicals as part of their policy of incorporation, the Left condemned the coalition governments as attempts to undermine the reform spirit of the republic. What Townson demonstrates in convincing detail is that the Radicals never lost their moderate republican agenda and that the much-maligned "black bienio" of 1933 to 1935 maintained many of the reforms instituted by the leftist coalition of 1931–1933. This was especially true until the socialist insurrection of October 1934 bolstered the position of the CEDA Right. But even after this event, the Radicals continued to defend, from their position of weakness, a commitment to moderate reform. Townson attributes to the Radicals' obstruction the fact that the CEDA proved unable to push through the majority of its counterrevolutionary legislation during 1935, with the exception of the Agrarian Bill of July 1935, which effectively reversed the agrarian reform of 1932.

If the Radicals did offer a real centrist option, then why did they fail to stabilize the republic? While Townson resurrects the Radicals from neglect, he does not glorify the party. He recognizes them as a flawed force whose contribution to the republic was ambiguous. In particular, he is critical of the party's historical leader, Alejandro Lerroux, who failed to offer the kind of vision and leadership that the party needed to negotiate its delicate balance, and who made significant political blunders. Other Radical leaders, like Diego Martínez Barrio, who quit the party in disgust in 1934, or Ricardo Samper, who led the last Radical-dominated government of 1934, seem to have offered more positive alternatives, leaving one to speculate on the role of individual personalities in determining the outcome. Townson also points to more institutional problems with the party, which never made the transition from a patronage-based political organization to a mass-based, bureaucratic modern party.

What brought the party down in the end was a corruption scandal in late 1935 of the kind familiar to the party, long associated with such dealings. But, as Townson argues convincingly, the scandal itself involved no more than a few bribes to public officials, nothing on the scale of the Stavisky Affair in France the year before. And yet, the Radical Party in France recovered from its more serious scandal while the Spanish Radicals collapsed. Townson points to a number of reasons for the adverse consequences, including a concerted campaign on the part of the Left, but the key difference was the willingness of the Spanish Radicals' right-wing allies to abandon them, thus demonstrating the weakness of the governing coalition. In more general terms, Townson criticizes the Left for treating the Radicals as the enemy, when they should have been pursuing a *modus vivendi* to preserve the republic. From the other side, the Right actively strove to undermine the centrist Radicals and replace them with an authoritarian government. The scandal and the Radicals' downfall was really a consequence of these two forces working together.

Townson's reinterpretation of republican political history is largely convincing. He makes an excellent case for the serious presence of a centrist force in the form of the Radical Party, while providing a nuanced explanation as to why that force could not help consolidate the republic. He even makes an effort to connect the narrative of national politics to the rest of the country by utilizing correspondence from local Radical leaders to Madrid. Although the material is not as extensive as one would like, Townson's attempt to gauge local responses to national politics is an important step toward creating a broader context for the rarified debates and machinations of politics in Madrid. One of the problems of the existing historiography is the gap between numerous local studies that reveal the tremendous diversity of Spain's political culture and the national political histories that seem to assume a uniform political culture. Given the lack of political integration and the uneven and weak presence of national political parties in the provinces, it is hard to imagine that different decisions made by leaders in Madrid would have been enough to overcome the serious obstacles to stabilizing a democratic republic in Spain in the 1930s. Thus, while the book's reinterpretation of national republican politics is entirely convincing, it does not necessarily resolve the broader debate about the fate of the republic. However, quibble over perspective does not diminish the importance of the book, which should become the standard political narrative of the republic for historians and students alike.

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MARGARET M. MCGOWAN. *The Vision of Rome in Late Renaissance France*. New Haven: Yale University Press. 2000. Pp. xiv, 461. \$50.00.

Margaret M. McGowan, the author of monographs on Michel de Montaigne and on Pierre Ronsard, now proposes this study of how Rome was understood by sixteenth-century French writers and artists. She tackles the vast topic by summing up, in succession, the reports of travelers, the substance of guidebooks, and the contents of princely collections of Roman antiquities, before moving on to more detailed accounts of Joachim Du Bellay's vision of Rome and Montaigne's ambiguous musings. Separate chapters on the perception of Julius Caesar and on the mania for constructing imitation Roman triumphal arches serve to underline the depth of French involvement in memories of the ancient Roman past.

The author faces an insurmountable difficulty: namely, the immensity of the subject. The ghost of the Roman past was everywhere. No painter, no architect, no engraver of the sixteenth century could sidestep the enveloping legacy of classical forms. Poets, dramatists, and men of letters in general were equally in the thrall of Roman models. McGowan limits her territory arbi-

trarily by considering only Rome's legacy, not that of Greece, and by describing only the French manifestations of the general engagement with classical antiquity. Even so, the bibliography is so extensive that much of her book reads like a recitation of the names of the massive cast she has chosen to audition.

Most of the time, McGowan, of necessity, depends on the enormous secondary literature devoted to this subject. Her method is illustrative: she refers to a great many artists and illustrates their contribution in over one hundred black and white reproductions of engravings and sketches, as well as twelve striking color plates. She calls upon a very large number of writers and, systematically, quotes illustrative passages from their books. Those quotations are given in the text, not in notes, and following each and every one of the French originals, the author provides an English translation. In this way, the reader's concentration is arrested on almost every page by double quotations that take up a great deal of space and make reading this book an experience resembling the reading of an exhibition catalogue.

Still, in spite of the fast-forward feeling produced by all those names spooling by, the book serves a useful function by directing the nonspecialist reader's attention to a number of key figures who took part in the dissemination of Renaissance classicism, including Sebastiano Serlio, Gabriel Symeoni (the chief source here is T. Renucci, *Un aventurier des lettres au seizième siècle: Gabriel Symeoni* [1943]), and Pirro Ligorio, who published only one book, *Antichità di Roma* (1553), but whose influence was profound. He crops up in several places in this fast-paced book, but neither Ligorio nor most of the other intriguing figures mentioned by the author come into focus. Fragments of information come before our eyes, fleetingly, like dust motes, only to return to their source, which is usually an erudite monograph. In the case of Ligorio, it is E. Mandowsky and C. Mitchell, *Pirro Ligorio's Roman Antiquities* (1963).

If there is an argument or something resembling a thesis in McGowan's book, it is barely detectable, offered as an afterthought in a six-page section entitled "Coda." The point made there is that the relationship between French culture and its classical antecedents underwent a fundamental change in the early years of the seventeenth century. For proof of this the author cites J. L. Guez de Balzac's letters (1624) to argue that "Rome had moved to Paris" and that "the imperial city was now embodied in the fabric of Paris" (pp. 350, 351).

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GEORGES VIGARELLO. *A History of Rape: Sexual Violence in France from the Sixteenth to the Twentieth Century*. Translated by JEAN BIRRELL. Cambridge: Polity Press. 2001. Pp. vi, 306.

Georges Vigarello has written a fascinating, sensitive, and nonsensational history of rape and sexual violence in France over the course of the last five centuries, concentrating on the eighteenth and nineteenth centuries. First published in France in 1998, this English translation is a welcome edition for all scholars interested in what society, the courts, the law, and the culture considered as rape or sexual violence. The definition, boundaries, meaning of these crimes, and sensitivities toward them varied over the centuries, depending on the age and vulnerability of the victim, the number of attackers, the degree of violence, the social status of both the victim and the attacker, and the historical era. This book is not just about rape; it is also a cultural analysis of the social construction of all crimes of sexual violence.

Throughout the centuries, law, judges, and society classified and categorized crimes, placing them in a hierarchical order according to degree of horror or threat to the public order. With a compassionate historian's consideration of the victims, Vigarello clarifies the complicated set of criteria that judges used to determine a rape or other crime of sexual violence. Showing greater suspicion of women than any understanding of them, however, judges assumed that women were inferior and weak-willed and that they voluntarily consented, unless there was visible evidence of bodily violence, such as knife cuts, witnesses to the woman's screams, or evidence that many men attacked her. It is no wonder that women were reluctant to speak out. Furthermore, until the late eighteenth century, society failed to recognize child rape. Since then, however, society has abhorred sexual crimes against children more than against adult women. Yet even these attacks on children were not prosecuted or understood, certainly not in their psychological or even physical ramifications, to the degree that they were in the late twentieth century.

In the world of the Old Regime, marked by a high tolerance of violence, the public ignored rape or treated it like other injuries. Threats to the social order, such as robbery, were more serious than threats to a woman's body. If the woman was young and from a propertied family with status, the sexual violence against her might have been taken seriously, especially if the rapist were a poor, marginal member of society. Conversely, if the woman were older, or a domestic servant, judges would not likely prosecute a rapist. Rape differed from other violent crimes in that it was also a moral offense. However, the victim was often the sinner, and child rape was not even a distinct criminal category.

Starting at the end of the eighteenth and continuing throughout the nineteenth century, French culture constructed violence differently, showing a decreasing tolerance for all violent crimes, including rape. Vigarello devotes a significant portion of this book to nineteenth-century changes. During this epoch, the frail child became a symbol of a sexual victim, and child rape emerged as a crime. Although women still

lacked legal rights, they increasingly went to the courts as rape victims after mid-century, although Vigarello does not make clear why. Nor is it clear why tolerance toward sexual crimes decreased. Trials for rape during the nineteenth century remained rare, but the popular press picked up the salient or significant cases. After mid-century, new words and concepts accompanied the decreasing tolerance of violence, with "attempted rape," "indecent assault," "offenses against decency," and "moral violence" entering the vocabulary and jurisprudence, especially as pertaining to attacks on children.

Nevertheless, doubts about the morality of the victim persisted, and doctors entered the scene to cast their opinion on the degrees of physical violence, the evidence for the women's acquiescence, and the personality of the accused rapists as degenerate men on the margins of society. Vigarello astutely shows that lesser charges might lead to conviction, while more rigorous charges might lead to acquittal. By the end of the nineteenth century, the public had become outraged by the rape and murder of children, with the press and popular literature helping create an increasing public sensitivity to these crimes and their consequences (although addressing the psychological pain of the child victims had to await the twentieth century). The book ends with the 1978 "trial of rape" in Aix and a change in the penal code in 1992 allowing for new definitions of sexual violence, including sexual harassment and rape by a husband of his wife.

It must have required intestinal and emotional fortitude to examine and retell so many of the archival court cases and newspaper accounts of such crimes. Criminal court records are often lurid, sometimes graphic. Vigarello is to be highly commended for undertaking this difficult research and for producing an important work on the subject.

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PHILIP T. HOFFMAN, GILLES POSTEL-VINAY, and JEAN-LAURENT ROSENTHAL. *Priceless Markets: The Political Economy of Credit in Paris, 1660–1870*. Chicago: University of Chicago Press. 2000. Pp. x, 350.

The subtitle of this book suggests that it is a general history of Parisian credit. It is not. Rather, it is a focused account of the rise and fall of the Parisian notaries who, in their heyday before the French Revolution, dominated the city's credit market to mobilize capital on an unprecedented scale. This book explains how these simple scribes ascended from financial obscurity to become stars of credit, only to end up on the periphery of finance in the nineteenth century. As they tell the notaries' tale, Philip T. Hoffman, Gilles Postel-Vinay, and Jean-Laurent Rosenthal offer fresh interpretations of historical events (the Law affair, the revolution, the emergence of the *Crédit Foncier*) and test economic theories about the development of

capital markets. The result is a lean book that is both conceptually sophisticated and deeply researched.

Simply recounting the story of the Parisian notaries and their role in the credit markets is a remarkable achievement. The notarial archives are vast, largely unindexed, and notoriously difficult to work with. Social historians have been known to dip into them, but Hoffman, Postel-Vinay, and Rosenthal, all seasoned specialists of French economic history, cast an unusually wide net, sampling from the millions of notarial contracts drawn up between 1660 and 1870. Gathering evidence on credit and manipulating the resultant data, the authors establish a chronology that begins deep in the Old Regime, crosses the revolutionary divide, and ends well into the nineteenth century. Although they fail to capture cycles of short-term credit (since short-term credit was not handled by notaries), the authors provide a masterful account of the evolution of long-term public and private credit over two centuries.

The story goes something like this. In the seventeenth century, Parisian credit stagnated, confined to a world in which personal ties (familial, commercial, and residential) held sway. After the Regency (1715–1723), however, notaries began to exploit their access to confidential financial information in order to arrange transactions between lenders and borrowers, facilitating a spectacular expansion of credit that would peak in the 1780s. The French Revolution dramatically reversed this expansion. Taking advantage of the revolution's high inflation, borrowers rushed to extinguish old debts and triggered the collapse of the long-term credit market. Private indebtedness did edge back up in the nineteenth century, but it would take six decades to reach its prerevolutionary peak; by that time the *Crédit Foncier* had taken over the Paris market, eclipsing the once-essential notaries.

The basic outlines of this plot are delineated in the second chapter, leaving the authors free to explore particular developmental phases in the rest of the book. The seventeenth and nineteenth centuries receive their due, but the heart of the book rightly concentrates on the most dynamic period: the "explosion" of private borrowing from 1726 to 1789. Using regression analysis, the authors contend that credit during these decades owed its meteoric rise to the matchmaking practices of notaries. Economic expansion and a stable currency were important factors, but notaries were instrumental in generating credit because they helped lenders and borrowers overcome a problem that economists call "asymmetric information." In a world where personal finances were shrouded in secrecy, notaries used privileged information to screen borrowers, match them with available lenders, and prepare suitable contracts. Matchmaking explains not only notaries' success before the revolution but their subsequent demise in the nineteenth century, when real estate transactions in the capital began to be registered publicly. Public registration eroded notaries' control over financial information,

forcing them to compete with one another and, worse, with banks. This new level of competition eventually drove the Paris notaries out of the market.

Whatever their ultimate fate, the tremendous success of eighteenth-century notaries calls for interpretation, and the book meets this challenge head on. The surge in notarial credit, the authors argue, demonstrates that large-scale capital markets existed long before the Industrial Revolution and the advent of modern banking. "If one believes that capitalism involves large-scale credit in a depersonalized market, then capitalism has to have arisen long before the nineteenth century" (p. 286). Moreover, the rise of credit in Old Regime France suggests that the emergence of large capital markets was not dependent on the development of English-style parliamentary government, as is often assumed. The Parisian credit market thrived despite the heavy-handed governance of French kings. Once again the vitality of prerevolutionary France surprises, challenging master narratives of financial history as well as theories of state formation and economic growth.

In addition to analyzing the long-term evolution of credit, the book provides a sketch of the men and women who composed the network of Parisian credit. The authors reconstruct a world that has been all but lost to historians, revealing the social background of lenders and borrowers, the role of women in the market, the influence of the life cycle on lending and borrowing, and the investment strategies of different kinds of lenders. A penetrating foray into the social history of credit, this part of the book will be as useful for historians of women, the family, and urban society as it will be for specialists of state finance and the economy.

Owing to the limitations of the notarial sources, however, the book's portrayal of the world of credit remains incomplete. First, the connection between credit and commercial investment, a subject of vital importance given recent work on consumer revolution and the commercialization of eighteenth-century French society, is never fully developed. Although the authors argue that the booming credit market allowed for greater investment in business and real estate, they cannot explore this question in any depth because so much trade was financed by unnotarized short-term debt. This is a pity. We can only hope that this book will embolden other historians to uncover the links between credit, commerce, and consumption in prerevolutionary Paris.

The book might also have taken the cultural dimension of the problem more seriously, examining attitudes toward credit with greater care. How did particular borrowers, lenders, and notaries conceive of credit and its dramatic expansion? Although we glimpse flesh-and-blood figures (Voltaire's financial correspondence is nicely invoked several times) and occasionally hear from writers such as Louis-Sebastien Mercier and Honoré de Balzac, attitudes toward credit are for the most part deduced from aggregate data and

interpreted within a market framework. Because lenders, for example, sought to preserve their capital and earn interest, the authors conclude that "the financial concerns of early modern Parisians were thus not remarkably different from our own" (p. 284). Historians who stress the otherness of the early modern period will probably bristle at this; I will simply point out that other forms of evidence must be introduced before such a statement can be considered definitive. Indeed, the authors themselves undermine their conclusion in a smart chapter on the French Revolution in which they explain the revolution's ruinous inflation by suggesting that revolutionaries subordinated personal fiscal interests to larger political and social agendas. Even in less stormy times, attitudes toward credit must have been fraught with considerations that could not be reduced to the bottom line.

Quantitative yet uncluttered with jargon, theoretical yet exhaustively researched, analytical yet committed to telling a story, this book represents economic history at its best.

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SIMON BURROWS. *French Exile Journalism and European Politics 1792–1814*. (Royal Historical Society Studies in History, New Series.) Rochester, N.Y.: Boydell. 2000. Pp. xvi, 272. \$75.00.

The French journalists active in London during the revolutionary and Napoleonic years were few in number, but Simon Burrows shows that their publications constituted an important aspect of the period's press and that, on occasion, they had a real influence on events. Britain had become a refuge for French journalists out of favor with their government well before the revolution, and the first French-language paper published there after 1789, the *Courier de Londres*, was simply the continuation of the long-established *Courier de l'Europe*, founded in 1776; it was not hostile to the revolution. As increasing numbers of émigrés crossed the English Channel, however, the papers they established took on a distinctly counterrevolutionary coloration. Until the end of the Napoleonic period, they served as an outlet for news and ideas that could not be published openly in France.

Burrows's thorough monograph situates the exile journalists in the wider context of the French émigré community, analyzes the political ideas expressed in their papers, and makes a case for the papers' political and diplomatic importance, especially in the Napoleonic period. Unlike the majority of the émigrés who settled in Britain, the journalists were for the most part neither nobles nor clergy, and they showed an aggressive entrepreneurial spirit in pursuing the business of counterrevolutionary propaganda, which brought at least some of them a respectable living. Thus they did not fit the standard stereotype of the émigrés as former members of the privileged classes living on handouts. Since the British government kept the exiled Bourbon

princes away from the capital, the journalists enjoyed considerable independence in expressing their views. Even when the British government subsidized them, it rarely interfered with the content of their papers unless they threatened its diplomatic undertakings. Small as it was, the exile journalistic community was divided between proponents of an integral return to the Old Regime and advocates of constitutional monarchy, although Burrows argues that the division between the two camps was not as unbridgeable as most studies of the counterrevolutionary movement have claimed.

The circulations of the émigré papers were limited and their audience was widely scattered, ranging from fellow exiles in Britain to political elites on the continent, but Burrows contends that they had some real influence. Publications such as J. G. Peltier's *Dernier Tableau de Paris* printed the first extensive accounts of events in France during the Terror, and these publications were also an important source of information about the uprising in Saint-Domingue, which sent its own crop of émigrés to London. The British government's unwillingness to silence the exile papers after the signing of the Peace of Amiens in 1802, Burrows claims, convinced Napoleon that a resumption of warfare was inevitable, while the French government's vehement complaints about these papers had the same effect on the British. Once Napoleon had imposed effective controls on the continental press, the exile papers were the only ones that circulated reports unfavorable to his regime in a language that European elites could read. Subsidies to these papers constituted a significant part of British propaganda expenses after 1803, even if the figures Burrows cites suggest that the royal ministers never really put much faith in the power of the printed word to destabilize "the Corsican ogre."

Although there have been biographies of key émigré journalists, such as Frances Acomb's *Mallet du Pan* (1973) and Hélène Maspero-Clerc's work on J. G. Peltier (1973), Burrows's book is the first comprehensive examination of the full range of exile papers published in Britain. It is regrettable that he did not extend his research to take in the French-language newspapers published in other European countries, such as the *Gazette de Leyde*, which remained a major source of uncensored news until the Dutch republicans muzzled it in 1798, and the *Journal de Francfort*, whose anti-French articles were extensively reprinted in the Paris press during the Directory. Burrows is correct that Napoleon succeeded in imposing a good degree of control over the French-language press throughout the continent, but periodicals published in German were less effectively reined in and were more important in keeping readers informed than he seems to realize. Burrows's effort to use the limited circulation of the exile press as evidence that there was at best a miniscule "public sphere" in Europe prior to 1815 is strained and unconvincing; the part of the population involved in politics and eager to follow the news

certainly grew in many parts of Napoleonic Europe, and in Germany in particular. Nevertheless, Burrows's work is a useful complement to the monographs on the counterrevolutionary press in France by William Murray, Jean-Paul Bertaud, and this reviewer, and it is a significant addition to the historiography dealing with French emigration.

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LINDA L. CLARK. *The Rise of Professional Women in France: Gender and Public Administration since 1830*. New York: Cambridge University Press. 2000. Pp. xiv, 324.

Rearranging the title of this monograph would have better conveyed its scope: the rise of professional women in French public administration since 1830. Such professions as medicine, the law, science and engineering, and the arts find no place here. Even the chief professional outlet for French women before World War I, teaching, earns treatment here (no doubt with some justification) not as a profession but as an object of supervision by female professionals. In this respect, some readers might be disappointed by the book's relatively narrow focus.

The narrow view has its compensations, however. Linda L. Clark has devoted intense study to the relatively small cadres of women in public administration. By focusing on available sources, she has been able to deliver convincing prosopographical analyses of successive cohorts, backed with sympathetic and nuanced portraits of individual women. The primary and secondary research undertaken by the author is staggeringly impressive.

The book is divided into two roughly equal parts. The first deals with the introduction and development of the "profession" of inspectors of nursery schools (*salles d'asile*), prisons, and schools, from the July Monarchy on, as well as inspectors of social and working conditions under the Third Republic, down to 1914. Female public administrators were originally considered necessary as a logical extension of the "substitute mothering" of the *salles d'asile* (later renamed *écoles maternelles*): only women could truly understand, and therefore administer, these social institutions (p. 19). The continuation of such attitudes, even among republican politicians after 1870, was part of a "backlash" that left achievements for women mixed down to 1914, when there were still only twenty-three female school inspectors in France (compared to forty-four in Britain; see p. 75).

The second part (including the chapter on the period since 1945) deals with the impact of wartime and interwar changes. Clearly World War I, with its massive mobilization of French males, occasioned a radical reassignment of women in the labor force, at least for the duration. Even though the National Assembly, following a trend in Europe and America, approved women's suffrage in 1919, its implementa-

tion was foiled by the French Senate in 1922. In the meantime, however, some national ministries—starting, perhaps ironically, with that of war—did start allowing qualified women to compete for the career rung of *rédacteur* (draft clerk or, as Clark translates it, chief editorial clerk), the "entry level rank in the hierarchy of the more prestigious civil service posts" (p. 141). Wartime exigencies had already produced floods of lower-level clerks, typists, and bookkeepers (many, however temporary) in government bureaus heretofore predominantly male. (Only the newer and more technically oriented branches of government, such as the postal, telegraph, and telephone branch, or PTT, had hired relatively large numbers of women as lower-level clerks.) The Ministry of War itself had relied on 16,000 women clerks in 1917 (p. 135). While the Ministry of War would later impose restrictions on women entering at the level of *rédactrice* and even more so on their promotion to higher administrative rank, "Ambitious women . . . after 1918 . . . found opportunities in ministries whose functions were not connected with older traditions of feminized public service or 'social housekeeping'" (p. 143). Yet it would be in such "soft" ministries as education, labor, health, and public works that women experienced better access and welcome, not in the traditional male (and elite) bastions of finance, interior, justice, the navy and—with a single regretted, experimental exception—foreign affairs.

An antifeminist backlash in the 1920s, the effects of the Great Depression (freezes and cutbacks), and the growing silent civil war between Right and Left by 1939 had left the following balance: of 621 *rédacteurs* in the nine French ministries with women civil servants, 210 were women. But only seventy-three had advanced to the level of *sous-chef* (out of 280) and only eleven (of 175) to that of *chef de bureau* (p. 203). Still, Clark sees this as a mixed bag of "both new opportunities and career disappointments," which surviving interviewees look back on in retrospect with "general satisfaction" (pp. 204–205). Clark supplies very interesting profiles of the background in social milieu (clearly mostly middle-class) and educational qualifications, as well as career patterns and such data as marriage frequency and children (lower than the average for French women). Helpfully, she also makes comparisons to other European countries (especially Britain, Germany, and Italy) as well as to the United States.

That women could crack the barriers of the civil service (and of course go on to make more numerically significant inroads in the past fifty years) is itself well worth a study of this quality. It should greatly interest readers not only as a contribution to gender studies, administrative history, and the (not always obvious) linkage between *mentalités* and social history but as a balanced, judicious, and well-researched contribution to general French history. The author's judiciousness sometimes also leads to an unwillingness to draw conclusions or make summaries, leaving judgments to the reader. And, to repeat, despite the title, this work

deals with a relatively small and narrow—if nevertheless significant—segment of French professional women in the nineteenth and twentieth centuries. Its strength also lies therein, for it can follow this segment over long time spans.

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DAVID ALLEN HARVEY. *Constructing Class and Nationality in Alsace 1830–1945*. DeKalb: Northern Illinois University Press. 2001. Pp. xii, 249. \$40.00.

This book surveys the evolution of political culture among workers in Alsace over the *longue durée* from the origins of Alsatian industrialization to the end of World War II. In two respects, Alsace is ideal for a study attempting to weigh the relative importance of class consciousness and nationality in the modern period. First, the department of Haut-Rhin was a very important center of textile manufacturing in nineteenth-century France, so the author can combine a traditionally defined working class with workers (such as artisans) more generally, dispersed throughout Alsace and more dominant in the department of Bas-Rhin. Second, the region's multiple changes in nationality—from French to German in 1871, German to French in 1918, French to German in 1940, and German to French in 1945—make the area an admittedly atypical test case where the development of national political culture, barely perceptible elsewhere, is put into relief for the historian's gaze.

To his credit, David Allen Harvey does not simply trace developments among the Alsatian working class, focusing instead on key events that reveal evolving national and class identities. He begins by considering the era before 1870, focusing on the revolutions of 1848. Harvey notes that, after the French Revolution, in which potential Alsatian republican sentiment was partly checked by the virulent Jacobin crusades against priests and the German language, interest in republican political solutions grew considerably. By 1848, Alsations, including workers, were among the most fervent supporters of the republic within all of France. Interestingly, Harvey finds that Alsatian workers' politics were heavily influenced by developments in the German states, so that their expressions of deep support for the French republic resulted from their knowledge of, and involvement in, a larger transnational political movement. As a result, in Alsace German-speaking people with largely German customs saw themselves as both French and republican.

Harvey then turns to the era of the notables, roughly 1850–1890, in which the largely Calvinist entrepreneurs in Haut-Rhin managed—in contrast to much of the bourgeoisie and the working class in both France and Germany after 1848—through their progressive paternalism to maintain a certain unity of pro-French, pro-Republican sentiment with their workers. Harvey is convincing in his periodization, making the case that a perceived unity of interests established before the

Franco-Prussian War continued during the early imposition of German control in Alsace. The anti-German protesters of the annexation found support among both the bourgeoisie and the working class after 1871. This section focuses more on Haut-Rhin, an emphasis that use of François Igersheim's work on notables in the Bas-Rhin might have balanced (*Politique et administration dans le Bas-Rhin 1848–1870* [1993]).

Between 1890 and World War I, notable control waned and Alsatian workers joined the now legal German Social Democratic Party (SPD). Harvey brilliantly shows workers who backed anti-German local notables before 1890 shifting their allegiance to the SPD, whose founders August Bebel and Wilhelm Liebknecht had condemned the annexation of 1871 and went to prison as a result. Although Harvey notes that class solidarity was the major reason for workers' interest in the SPD, an international class consciousness in the era of the Second International that Alsations took very seriously, a vote for the SPD was also an anti-Prussian protest vote. This unity held until World War I, when Alsations' place in the German Empire no longer needed to be accepted as a fait accompli. More to the point, German military rule in Alsace during World War I managed to undermine the tacit acceptance of German nationality that had emerged before the war.

Harvey then traces the shifting Alsatian views of France and Germany, describing French heavy handedness in the 1920s, the autonomist movement, and working-class positions during the interwar years. Here Harvey distinguishes less between the workers and other Alsations, moving back and forth to tell the broader story of Alsatian political developments in the twentieth century. Yet more than many other Alsations, workers no longer saw reaffiliation with Germany as a viable option once Adolf Hitler came to power. After the Nazi occupation of Alsace (1940–1945), working-class support for an increasingly worker-friendly French state (which Harvey dates from the Popular Front, then traces into the Fourth and Fifth Republics) was secure. Today's European internationalism, rooted in Franco-German cooperation and committed to democratic welfare states, is in essence what many Alsatian workers had wanted all along.

Despite its brevity, this book is basically free of oversimplifying generalizations. I would disagree with Harvey's assertion that "efforts at [linguistic] assimilation came to an end soon after 9 Thermidor, and would not be seriously attempted again until the twentieth century" (p. 16), as it understates attempts by both the French and the Germans to use schools for linguistic assimilation, but that is a relatively small matter. I was initially more troubled when, on four separate occasions, Harvey referred to the Alsations as "fickle" (albeit combined with the word "supposedly" in the conclusion) in their national identity, a term that itself assumes Alsations needed to be clearly either/or in defining their national loyalties. In his introduction,

Harvey is absolutely right in arguing that national political cultures cannot be taken for granted, and by combining a consideration of a working class in Europe with a regional study of national identity, he has helped to take us part of the way toward a conceptualization of European history that is not solely defined by the nation-state. The fact that Harvey himself has an occasional lapse reveals that we still have a ways to go in reconceptualizing our approach. Of course, Harvey is not singlehandedly responsible for the revision of a historiography far too willing to focus on the nation-state; would that all European historians had thought through the issues as well as he has.

Given the title, some historians will expect Harvey to have gone farther in studying the construction of class and national cultures more generally, including the bourgeoisie as well as the working class. He does include a really interesting discussion of social democratic subculture in turn-of-the-century Alsace, a kind of analysis that has largely been lacking for Alsace. The sources for Alsace are rich; I very much hope that Harvey and others might eventually consider even more closely the interactive nature of the discourse of emerging class and national identities. To oversimplify: how did the bourgeoisie define itself in relation to the working class or peasantry, and vice versa?

Both the author and the press deserve praise for this volume. After recently reviewing several other books, it was a pleasure to read one completely untainted by either grammatical or typographical errors. This is a good, well-written, and well-produced book.

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JONATHAN BEECHER. *Victor Considerant and the Rise and Fall of French Romantic Socialism*. Berkeley and Los Angeles: University of California Press. 2000. Pp. xvi, 584. \$65.00.

Victor Considerant (1808–1893) is remembered as the most influential disciple of the utopian theorist Charles Fourier. Jonathan Beecher, the author of what will long remain the definitive biography of Fourier (*Charles Fourier: The Visionary and His World* [1986]) has now published as well-researched and thoughtful a work on Considerant. Not that Beecher's Considerant is simply a sequel to his Fourier. Considerant's life and ideas are considered in their own right without the "amused condescension" (p. xi) with which he and his generation of reformers have long been treated. Nor does Beecher patronize Considerant as a precursor to subsequent, more sophisticated or "scientific" movements for radical reform.

Building on indefatigable and ingenious archival research, Beecher follows Considerant's lifelong political journey from his youthful apprenticeship as a Fourierist ideologue, through his leading role in the socialist movements of the 1840s, his participation in the political failures of the *démoc-socs* of 1848 and 1849, his years of Belgian exile, his attempts to estab-

lish a Texas utopia, and the long years as a venerated, or ridiculed, *vieille barbe* of the Romantic era. Beecher's sympathy is tempered by a scrupulous awareness of Considerant's manifest flaws as a thinker and leader. We get Considerant, warts and all, yet without the least indulgence in irony at the expense of this "steadfast and stubborn" optimist (p. 281). Irony is certainly tempting when considering the case of Considerant, who defended with eloquence, charm, and charisma a succession of falsified prophecies and unsuccessful projects.

While the master was still alive, and a thorn in the side of his supporters, Considerant managed to emerge in the first rank of the disciples, and, after Fourier's death in 1837, he assumed the role, not without opposition, as the leading interpreter of the doctrine. Those internecine struggles over Fourier's legacy actually had less to do with correct exegesis than with the tactics appropriate to the current situation. Virtually everyone agreed that it was prudent to leave buried in manuscript Fourier's most shocking sexual views and his wild fantasies about the universal transformations that would infallibly follow the rational ordering of human society. Unfortunately, vigilant journalists managed to unearth Fourier's prediction of a new member of the human body, the *archibras*, or long tail with a small hand at its extremity. Substituting an eye for the hand at the end of the tail, the media affixed the tail to Considerant in all of those sardonic cartoons depicting members of the socialist rogues' gallery.

Despite his leading role in preaching Fourierist doctrine, Considerant never did find the time quite right for the establishment of an actual phalanstery, the model community to be organized after Fourier's blueprint. He believed that the ground had first to be prepared by education, lecturing with considerable success to any available audience and founding a series of journals. *La Démocratie pacifique*, launched in the 1840s, made a significant contribution to the rich literature of what Beecher identifies as Romantic Socialism. During this period, Considerant lifted his eyes beyond the horizon bounded by Fourier's particular nostrums to a vision of socialism as an "immense, irresistible aspiration toward the emancipation from the wage system and the establishment of a harmonious and peaceful society" (p. 232).

Beecher's treatment of this stage of Considerant's career in the context of the era of Romantic Socialism is especially impressive. His characteristic empathy for generous aspirations never descends to apologetics for the lack of political realism that contributed to the movement's demise. He unsparingly exposes Considerant's unfounded optimism in 1848 and his feckless political behavior, notably on June 13, 1849, when the *démoc-socs* and Left Republicans, outraged by the French attack on Giuseppe Mazzini's Roman Republic, threatened insurrection without doing anything to carry it out. This provided the excuse for the repres-

sion that sent many socialists and republicans, including Considerant, into exile.

Considerant left his country in 1849, not to return permanently to France for twenty years. He lived in Belgium under intermittent surveillance for the first few years, and then in 1852 set off to discover America, and, especially, to discover Texas. Or, one might say, to imagine Texas, as his reports on that promised land for communal experiment—"in no other part of America was rainfall more reliable than in Texas" (p. 315)—rather romanticized that infernally difficult environment.

The effects of his brilliant salesmanship would outrun his intentions. Considerant's descriptions of the region he identified for the establishment of experimental colonies struck a chord in Fourierist circles in France that released a flood of immigrants far larger than Considerant had intended, or that his selected region could support. His pilot community would wither from drought, overcrowding, bad luck, agricultural incompetence, and Considerant's failures as a leader. A remarkably effective publicist, when the going got tough Considerant would fall into chronic despondency or leave the scene. In a broader sense, his was one more unsuccessful attempt to right the wrongs of an emergent industrial capitalism through the establishment of a model agrarian community.

After two aborted Texas projects, Considerant hung on in Texas as a marginal farmer, managing to get himself trapped in a manifestly unsympathetic South during the Civil War. After his return to France in 1869, he lived on into old age as a relic of a distant era and discarded ideals. As Beecher concludes, "Considerant's failures were part of a shared experience. For a whole generation of republicans and Romantic Socialists, the defeats of 1848–1851 served to shatter the hope that the world might be remade through politics, that the coming of democracy would lead to a new age of individual regeneration and social harmony" (p. 450). Beecher's humane sensibility and critical acumen help us to understand those lost hopes and that remote, but not completely unintelligible, era.

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BERTRAND TAITHE. *Defeated Flesh: Medicine, Welfare, and Warfare in the Making of Modern France*. Lanham, Md.: Rowman and Littlefield. 1999. Pp. x, 292. \$67.00.

This book is a meditation on war, set within the Franco-Prussian War and the Paris Commune (1870–1871). Bertrand Taithe argues that war is neither chaos nor battles and heroism but a different kind of life, that war brings about a redefined social and political order. His goal is both to provide a general historical account and to recover individual recollections of war in order to examine how memory reconstructs the experiences of war and defeat. Using archives that survived the Commune fire of May 1871, war stories,

and a variety of printed sources, Taithe aims to convey a personal and civilian experience of war and siege.

The first two-thirds of the book deal with war, with an emphasis on the siege of Paris and the Commune, and the last third focuses on the experiences of defeat. Taithe begins with a review and analysis of the events of 1870–1871, considering historiographical debates over the origins of the war and the Commune and the causes of French defeat. He then moves to a description and analysis of medical France in 1870, emphasizing prevalent diseases, morbidity and mortality, hospitals, and the politics of medicine. Taithe portrays Paris as a "giant hospital" in which almost any available space was turned into an improvised ward. Both governmental and nongovernmental agencies cared for the wounded and sick, as Paris was mobilized into both a healing and a war machine. He shows how the militarized "giant hospital" was doomed to failure when institutional rot and human decay set in, demonstrating that the moral order of military medicine could not survive in unsanitary, besieged Paris.

In a chapter devoted to provisioning Paris with food and heat during the siege, Taithe provides a vivid account of ersatz and replacement foodstuffs proposed by the Academy of Sciences and industrialists. He portrays the Parisian government as helpless in providing food and fuel, however, due to accepted limits of state intervention in a free market. Paris starved for over 133 days.

The last third of the book considers what defeat meant for the French people. The Prussian defeat seemed to justify the gloomy analysis of national character and medical causes of decline offered by physicians and reformers. According to Taithe, defeat came to symbolize all that was wrong with France, confirming feelings of inferiority and degeneracy. The language of defeat focused on specific problems and diseases, such as alcohol, venereal diseases, and tuberculosis. The prevailing notion was that France was pathological at the core, and a sick nation could not defend itself. None of this thinking was new. Rather, war and defeat intensified a discourse that dated from the early nineteenth century when hygienists considered Paris a sick city, and more recently, from the 1850s with the articulation of degeneracy theory.

Taithe's thesis is that the defeat of the flesh became the central metaphor by which the French represented war after 1870. He explains that "their collective failure was the sum of their individual deficiencies. The defeat was in their flesh" (p. 237). He argues that in the ensuing decades "defeated flesh" remained an important metaphor and analytical tool, supported by medical evidence and lending strength to narratives of French history. The notion provided the energy and rationale for vindication (*la revanche*).

This book is part of Taithe's long-term project on war and modernity, whose theme is that the Franco-Prussian War shaped modern France, because it played a central role in citizens' lives as they reflected on the experience and consequences of war. While this

book stands alone, it can be better appreciated when read as a companion piece to Taithe's recent work, *Citizenship and Wars: France in Turmoil, 1870–1871* (2001), which takes the war outside Paris, providing a broader coverage in which Paris figures as a case study.

The book succeeds admirably as a meditation on war and an analysis of war, medicine, and society. Taithe vividly recreates the social and personal history of war as lived by civilians. All in all, Taithe provides a stimulating account that will be appreciated by historians of war, modern France, medicine, and public health—even though the book covers well-trod territory in the history of medicine. This work has an even broader significance after September 11, 2001, as we reconsider the role of war in shaping citizens, national identity, and modern society. Taithe's account is timely and reminds us how war, like disease, can redefine a nation. The book is enhanced with many illustrations and tables, and a detailed bibliography is a welcome addition.

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JACQUES PORTES. *Fascination and Misgivings: The United States in French Opinion, 1870–1914*. Translated by ELBORG FORSTER. New York: Cambridge University Press. 2000. Pp. x, 454. \$54.95.

Jacques Portes read over five hundred books, mostly travel accounts but also political essays, sociological studies, and the like, and scoured hundreds of magazine and newspaper articles in order to discover what the French thought about America between the 1870s and World War I. His effort, first published in French in 1990 and now translated by Elborg Forster, won the annual prize awarded by the Society for French Historical Studies for the best book on Franco-American relations.

In both concept and method, this is a traditional historical study. The topic is public opinion, the research medium is the printed record, the writing is devoid of jargon, and theory is absent. The author's judgments are nuanced and informed, and he is judicious in his use of the category of public opinion. To Portes, "French opinion" means the writings of several hundred men, and a few women, from the privileged classes, most of whom spent a few months touring certain regions of the United States. He shows, in addition, that even among this select sample there was no single "French opinion" of America because attitudes were so diffuse. In fact, the complexity of these responses can frustrate a reader who assumes there was a "French" view of many topics.

The traditional character of this book does not mean, however, that the author ignores topics of contemporary interest. Portes gives considerable attention to such topics as women, race, immigration, and imperialism, and he discerns important parallels between the late nineteenth century and the present, arguing that one can find in this period the pattern of

perception that formed the basis of twentieth-century French anti-Americanism. But these early observers were not anti-American. They were ambivalent about what they saw because America both fascinated and disappointed. On the one hand, the French, at first blush, discovered the future on this side of the Atlantic: vibrant big cities, widespread material comfort, and wondrous efficiency. On the other hand, upon closer examination, they were dismayed at finding brutality, vulgarity, waste, greed, and commercialism. Only a few condemned America outright; most offered mixed assessments.

Portes structures opinion around three themes: the United States as a site for the exotic, as a model society, and as an emerging power. In the first section, one sees the French shifting their interest from "exotica" like the Wild West to New York City. In the second section, Portes reiterates the question posed by these visitors: did American democracy or social relations offer a model to the French? In general, except for religious toleration, the answer was mostly negative. And, in the final section, Portes asks whether or not the French saw American power in its economic, imperial, and cultural dimensions as a threat. Here again the answer is negative. The French may have been impressed with the mechanization and output of the American industrial plant, but they worried more about issues like the trusts and the morality of lax bankruptcy laws than they did about the potential of American industrial growth. Whatever interest they showed in American overseas expansion declined after the Spanish-American war. French observers concluded that the United States was on the way to becoming a world power, but they did not find either its economic system or its social heterogeneity admirable, and they believed the American way of life to be based on values that were opposed to the emergence of true culture (i.e. art and literature).

The posture of the French in these decades was that of detached observers: America was an interesting experiment that might have some bearing on their future. But the New World was of marginal interest, and it did not appear as either a model or a threat until after 1914. Nor did the French fully grasp what was happening in the United States. They often underestimated and misunderstood the significance of certain developments. For example, the French seemed blind to the importance of mass immigration. In this entire period not one book, or even a complete article, was published by a French author on the subject. Nor did the French perceive the future of mass culture. In many cases it was their own experiences—e.g. with immigration, empire, or culture—that clouded their vision of what was happening in America. French visitors not only missed much, but they also often oversimplified and harshly judged what was different. In general, these observers displayed a strong predisposition for social hierarchy and traditional social roles. If they admired American education, for example, they also criticized it for encouraging higher

education for women. Despite the impressive volume of writing, America was of little immediate significance to the French. Thus not one famous author, intellectual, or major statesman (André Tardieu aside) wrote about America during this period.

For a student of modern France, Portes demonstrates the existence of a standard language about America, full of clichés, images, and judgments (some positive, many negative) that was fully articulated by 1900. Subsequent generations may have embellished this appreciation of the New World, but much of the original has survived. In the current debate about Americanization and its twin, globalization, one can recognize this early discussion about America. And the final judgment, then and now, is the same: despite American dynamism, power, and prosperity, the French conclude “we’re better off in France.”

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CHRISTOPHER E. FORTH. *Zarathustra in Paris: The Nietzsche Vogue in France, 1891–1918*. DeKalb: Northern Illinois University Press. 2001. Pp. 238. \$42.00.

Since the turn of the nineteenth century, Friedrich Nietzsche has been a periodic favorite of avant-garde litterateurs and philosophers in France, from André Gide to Jean-Paul Sartre to Gilles Deleuze and Félix Guattari. Why should this be so? Christopher E. Forth takes a close look at the first Nietzsche vogue, launched in the 1890s, and proposes the outlines of an explanation.

Forth’s answer comes in three parts. The first touches on the peculiar constitution of French intellectual life, and on this score Forth follows the analytic lead of Pierre Bourdieu. Up-and-coming thinkers and writers in the *fin de siècle* confronted an entrenched institutional complex, organized around the twin poles of the university and the major literary reviews. Late nineteenth-century French academe was populated by high-minded republicans, partial to the moral rigors of Kantian philosophy. The literary establishment was more conservative in cast, less preoccupied by the demands of the categorical imperative than by the corrosive effect of cosmopolitan influences on French letters. What a perfect foil Nietzsche made in such circumstances: a herald of joyous self-assertion, a German by birth but a European by avocation, he might be called on to raise the hackles of right-thinking moralizers of all kinds.

And so it was. Free-thinking aesthetes gathered round little magazines like *La Revue Blanche* and *L’Ermitage*, the young Maurice Barrès, such were Nietzsche’s first acolytes in France. They lived on the margins of intellectual life, but what they lacked in institutional power they made up for in youthful energy, artistic purpose, and an encompassing disdain for philosophic and literary pieties.

Part two of Forth’s explanation turns to politics. French Nietzscheans made their home on the edges of

a resurgent Left. In the 1890s, they flirted with a libertarian anarchism, and at the time of the Dreyfus Affair, most took the side of the unfortunate Jewish captain.

Post-Dreyfus, Nietzscheanism in France veered in a variety of directions. There were revolutionary syndicalists, contemptuous of all humanisms, who looked to Nietzsche as the prophet of action-minded *minorités agissantes*. The Right, too, had its Nietzsche. Here, after all, was a Francophile philosopher (and Nietzsche did champion French culture, the better to irritate vulgar German nationalists) who called for a new aristocracy, energetic and willfull, dedicated to regeneration through beauty. This was the Nietzsche that the Action Française impressed into service against the flabby decadence of a mongrelized, republican France. Such illiberal appropriations, however, are not Forth’s main concern.

The true heirs of 1890s Nietzscheanism, he argues, were not so much Sorelians or Maurrassians who placed politics first as the avant-garde litterateurs of the *Nouvelle Revue Française* (NRF). The trajectory traced by the *L’Ermitage* circle bears out Forth’s point. Dreyfusards they became, but an ongoing Nietzscheanism persuaded most to return to the aestheticizing ways of old. This was the case of Gide, who published *The Immoralist* in 1902 (the beyond-good-and-evil title betrays the novel’s Nietzschean sources) and then went on in 1909 to found the NRF, soon to become France’s premier journal of modernist experimentation.

For French Nietzscheans, the borderline between art and politics was easy to cross and recross. In the heady days of the *fin de siècle* and early Belle Époque, commitment most often took left-wing forms, anarchist or Dreyfusard. Such engagements were unstable to be sure, but Forth’s point remains: French Nietzscheanism carried a leftist charge, however immanent, however volatile. The ultimate fate of the first Nietzsche vogue would seem to clinch the argument. It was done in by the National Revival of the prewar years and then, during the war itself, by the unbending anti-Germanism of the Union Sacrée. In France, the Nietzschean impulse had a hard time making its way when the prevailing climate was rightist.

Forth’s third line of argument has to do with gender. The literary establishment of the 1890s smeared Nietzscheans as effeminate neuropaths. The Nietzscheans, however, understood what they were about in different terms. They rejected the tired-out decadence of symbolists and Wagnerians, proclaiming themselves the apostles of a new manliness, youthful and vigorous. It was a position, however, they were not able to maintain. After the turn of the century, women novelists like Anna de Noailles turned to Nietzsche, invoking him not to score points against decadents but to dig away at the conventions of bourgeois morality. Gide and his fine-chiseled immoralism lacked the firepower that the early Barrès, prince of youth and self-styled professor of energy, had had in the 1890s. In the meantime, National Revival partisans made a display

of their own virility. The young men of today, they declared, liked practical things such as sport, adventure, exploration, not art-for-art's sake intellectualism. At the turn of the century, the Nietzschean avant-garde had a fighting chance for the mantle of manliness; by the war's eve, it had lost the battle. The National Revival then trumped the Nietzscheans twice over, first as German sympathizers and then again as head-in-the-clouds aesthetes.

The avant-garde intellectual at loggerheads with both academic and literary establishments, the libertarian at a moment of leftist reawakening, the professor of manliness at a time of intellectual decadence: such was the stuff that the first French Nietzscheans were made of. Forth's analysis does not answer all questions. Why was manliness, however defined, such a critical virtue to avant-garde modernists? And what about Nietzsche's Hellenism? German critical philosophy has a marked weakness for Greece, Nietzsche for Greek tragedy, and, later, Martin Heidegger for the pre-Socratic philosophers. Did French Nietzscheans and Heideggerians share in this sympathy, and what were the meanings of Greece to them, inhabitants of a nation so proud of its Latin heritage?

That said, Forth's book proposes an engaging and suggestive multidimensional analysis, inviting readers to imagine how it might apply to subsequent Nietzsche vogues. Indeed, the book poses a yet larger question: whether the particular institutional and cultural configuration that made Nietzsche such a perennial on the French avant-garde Left in the twentieth century still remains in force today, enough so to permit Nietzsche, that philosopher of eternal return, to come back yet again and again in the century that lies ahead.

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GARY K. WAITE. *Reformers on Stage: Popular Drama and Religious Propaganda in the Low Countries of Charles V, 1515–1556*. Buffalo, N.Y.: University of Toronto Press. 2000. Pp. xxii, 364. \$75.00.

The Chambers of Rhetoric or *Rederijkerkamers* were a peculiarly Dutch institution that has long been thought influential in the spread of evangelical thought. The plays they wrote and performed—often in competition with chambers from other cities—have been identified as important vehicles of evangelical sentiment in that crucial period when formal expressions of support for Martin Luther's movement were closely watched and heavily punished. Gary K. Waite's carefully researched and long-digested study is the first complete investigation in the English language of the social composition and influence of the chambers, and as such it is extremely welcome.

Inevitably, given their high profile in the three decades between 1520 and 1550, the plays of the *Rederijkers Kamers*—particularly the sensational national competition of 1539, the *Landjuweel*—have attracted much interest, particularly from literary schol-

ars. Many of the texts are available in modern editions. The two great virtues of Waite's book are that he offers a systematic analysis of the surviving plays' religious orientation and anchors the chambers convincingly in their local social context. He is able to draw a fascinating contrast between the social milieu of the *Rederijkerkamers* in Antwerp and Amsterdam. In both places the chambers attracted "a good cross-section of the upper artisanal, professional and merchant elements of urban society" (p. 39). Their members included many who had held civic office and members of other important social institutions, such as confraternities and militia companies. But in sophisticated, cosmopolitan Antwerp, the upwardly mobile membership embraced modernizing tendencies within the drama. Some plays introduced classical themes; for some rhetoricians, the old-fashioned allegorical religious plays (*spelen van zinne*) were not sophisticated enough. The advocacy of evangelical themes was subtle and restrained, and in consequence the chambers maintained a good relationship with the magistrates, who encouraged the actors and provided financial support. In Amsterdam, at this date a much smaller, less prosperous, and more provincial city, the *rederijkers* became much more vigorous proponents of religious reform. The Amsterdam rhetoricians did not enjoy the support of the city fathers, and in consequence were likely to turn to other groups, such as the artisanal guilds. Their flirtation with revolutionary Anabaptism in the attempted coup of 1535 demonstrated that the marginalization of the city's *rederijker* culture had ultimately been more dangerous.

Waite's investigation of the reform themes embedded in the plays reveals a wide spectrum of religious views. Most evinced a modest reform evangelism, more or less consistent with orthodoxy (depending on the climate of the times and the interpretative bent of the audience). Some were far more radical. The *Landjuweel* of 1539, coming shortly before the Ghent revolt, was a truly seminal moment, revealing as it did the extent to which the center of gravity of the religious views of the prosperous urban laity had drifted away from strict orthodoxy. But despite the severity of the clampdown that followed, the plays continued after 1540 to promote criticism of orthodoxy and advocacy of evangelical ideas, albeit with greater subtlety and concealment.

In the last fifteen years of Charles V's reign, Waite also discerns signs of increased Calvinist influence. A number of plays continued to promote religious orthodoxy, even criticism of evangelicals. But for the most part—and this is a significant finding—"even Catholic playwrights constructed their plays in ways that defended a traditional understanding of salvation and sacramental faith without unduly alienating reform-minded members of an audience. This could be done by virtually ignoring contentious theological issues, and/or by so highlighting a criticism of clerical abuses that reform-minded viewers could still appreciate the play" (p. 203). Even Catholic authors criticized mate-

realistic piety and clerical abuses, revealing the extent to which impatience with the clergy and skepticism of the capacity of the organized church to lead reform had spread across the religious spectrum. Perhaps this was not surprising. The plays of the Chambers of Rhetoric catered to the tastes and exposed the prejudices of a self-confident and assertive social class that was both politically powerful and numerous in the unusually urbanized society of the Netherlands. Waite's book reveals a sustained interest in themes of religious reform that, while they could not in this period promote an official Reformation, nevertheless helped prepare the way for the more tumultuous events of the second half of the century in the Netherlands.

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MARCO H. D. VAN LEEUWEN. *The Logic of Charity: Amsterdam, 1800–1850*. Translated by ARNOLD J. POMERANS. New York: St. Martin's. 2000. Pp. 242. \$69.95.

In an attempt to deescalate social unrest over the closure of the Almoner's Orphanage in Amsterdam in 1824, the city fathers wrote the province of Holland that "the Almshouses, and especially this institution as a labor of love, afford proof of the unstinting generosity of the people of Amsterdam and of the fact that God's blessings are bestowed on such labors of love and on the citizens who perform them" (p. 181). Historiography on poor relief in nineteenth-century Europe, however, has shown that statements like this must be placed in their social and economic context. Accordingly, Marco H. D. van Leeuwen argues that poor relief has to be understood as a factual interdependence between the elites and the paupers. Through poor relief, the wealthy offered the poor basic support in exchange for both their labor and their loyalty. Thus, the elites prevented workers who were essential to economic activities in Amsterdam from migrating from the city. Furthermore, poor relief disciplined individuals to adhere to the established social order. The poor, on the other hand, received a minimum guarantee for physical survival, both in the form of money and goods and in terms of facilities. Their children visited charity schools in order to learn how to read and write and were vaccinated against the dangerous smallpox.

Van Leeuwen explains the interdependence between upper-class citizens and "paupers" in Amsterdam, which is the focus of his study, by using what he describes as "a simple model of poor relief in preindustrial Europe." This model, based on Raymond Boudon's work on "goal-oriented behavior," assumes the investments necessary to realize poor relief had to be balanced by the interests of the elites. The poor, in turn, had to be willing to receive relief instead of alternative survival strategies such as migration, begging, crime, prostitution, or revolt. In this way, poor relief existed in a precarious balance, the consequence

of which was the duty of elite families to provide assistance to the poor and the right of the poor to utilize the support offered to them. In his model, van Leeuwen intends to incorporate the effects of poor relief both in terms of the increase or decrease of the number of poor and in terms of (de)regulation of the labor market and (de)stabilization of social order. Thus the model, according to the author, permits "three simple questions" to be answered: which groups provided poor relief and why? Which groups received poor relief and why? What effects did poor relief have on society?

Through a thorough analysis of historical sources based on his model, van Leeuwen opens an interesting insight into the world of poverty and richness in Amsterdam in the first half of the nineteenth century. We learn about the long Dutch tradition of taking care of the poor by providing them with outdoor and indoor relief. The author outlines the strong social segregation in Amsterdam, which, however, did not shield the rich from a life spent face to face with the poor. This social closeness undoubtedly enhanced a system of poor relief that was able to function as a strategy of control in order to protect the living conditions and the wealth of local elites. Although historical sources provide much evidence for his argument, van Leeuwen is also forced by his data to modify the general picture resulting from his model. In addition to members of the elites, for example, many middle-class Protestant men appeared to be involved in poor relief, albeit under the surveillance of the Protestant elites dominating the Amsterdam church hierarchy. Furthermore, van Leeuwen's sources show that women outnumbered men among the recipients of poor relief, whereas children and elderly outnumbered adults between the age of twenty and sixty-nine. This, however, seems to have no serious implication for Van Leeuwen's perspective, which remains strongly focused on the social and economic factors, in particular that of a reserve labor market.

Van Leeuwen's emphasis on the "homo economicus"—mainly perceived as a male identity throughout the book—is both the strength and weakness of this well-written study, which undoubtedly adds a great deal, in particular about the Dutch case, to the rich international historiography on poverty and social care in Western Europe. Notwithstanding van Leeuwen's own warning about jumping to easy conclusions, however, the book fails to consider systematically a perspective that goes beyond "interests." Van Leeuwen could have been inspired by Frances Gouda's remark in *Poverty and Political Culture: The Rhetoric of Social Welfare in the Netherlands and France, 1815–1854* (1995) that "civic culture of the Dutch nation . . . could only encircle its impoverished members in a gentle embrace if it was firmly grounded in, and shaped by, religious sentiments" (p. 180). This insight might have kept van Leeuwen from putting "pure altruism" aside, because it "cannot be incorporated into a model of goal-directed actions" (p. 10). Even an economic

approach can be sensitive to cultural arguments, as has been shown, for example, by Thomas Haskell's concept of "humanitarian sensibility." An approach more open to both gender and cultural or religious factors would have offered the incentive to rethink the "labour of love" of the wealthier citizens in early modern Amsterdam in a new way.

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BIRGITTE SØLAND. *Becoming Modern: Young Women and the Reconstruction of Womanhood in the 1920s*. Princeton: Princeton University Press. 2000. Pp. x, 249. \$35.00.

In this short, clearly written book, Birgitte Søland examines how Danish women who came of age in the 1920s reshaped their female identities and gender relations so that they might lead what they regarded as "modern" lives. It was a time in which a reorganization of gender relations was occurring in much of Western Europe. In the analyses of causes for this reorganization produced to date, World War I conditions in the belligerent states have been the focus, particularly the impact on women of wartime industrial work and on men of military experiences. The absence of these causes in neutral, largely agrarian Denmark makes Søland's study a worthwhile addition to efforts to make the European women's history record more complete. That her book is one of the few carefully researched historical studies of twentieth-century Scandinavian women published in English is also welcome. It will assist in clarifying how Scandinavian women by mid-century were regarded internationally as having achieved a high level of equality with men.

In order to establish that Victorian gender arrangements ended and a new gender order emerged in the Denmark of the 1920s, Søland commences by devoting attention to the reactions of ordinary working-class and middle-class people of all ages to the behavior of young unmarried women. They were discarding old notions of propriety, shortening their skirts, wearing makeup, playing sports, going out at night, dancing, and flirting. And once married, they continued to assert their identities as "modern" women. Søland emphasizes the rapidity with which such behavior appeared from 1919 on and concludes that there is little evidence that the Danish women's movement, founded in the nineteenth century, or the new Marriage Act of 1920, with its pioneering section on the equal economic status of spouses, provoked it.

Søland bases her findings on a wide array of archival and primary sources, ranging from parliamentary, legal, and statistical records to newspapers, women's magazines, popular fiction, and personal advertisements. She also has studied hundreds of unpublished autobiographical accounts found in Danish archives and has personally conducted fifty-nine oral history interviews with women born between 1895 and 1916, all of whom worked for wages during their youth. She

has written up her findings in an enjoyable way and interspersed them with well-selected photographs, cartoons, and posters of the period.

In a carefully structured succession of six chapters, Søland first describes the styles adopted by young women of the 1920s in dress, coiffeur, and posture so that they might acquire a "modern" look, and their rejection of domesticity in favor of going outside the home to work and to play alongside men. A chapter is devoted to the difficulties in relationships with young men to which these changes gave rise. The last two chapters deal with debate on the nature of marriage and housewifery, young Danish women of the 1920s never rejecting marriage and the financial dependence on a husband that came with it. Still determined to assert their identities as "modern" women, they attempted to elevate the status of housewifery to one that required "real" skills by reaching out for courses, fairs, and educational literature that would give them training in these skills. Among their strategies was keeping records of household expenses that could be used when discussing family finances with their husbands. In general, they were moving toward securing a broader set of economic rights within their marriage.

Some of the younger married women, recalling how the wartime Danish government had praised women for their contribution to the national economy, pointed out their contributions as skilled housewives to the troubled Danish economy of the 1920s. Discussion of this and other aspects of the ways in which married women aspired to achieve greater economic equality with their husbands constitute rewarding portions of Søland's book. Also impressive is the treatment in chapter five of the extent to which young married women realized the ideal of happy, fulfilling partnership in companionate marriages. Readers will find little information on how women participated in the political arena, if at all, in their pursuit of heightened status. Elaboration on the brief reference to the appointment of housewives to sit on political committees would have been helpful.

By avoiding overly lengthy explanations of background causes, Søland succeeds in keeping her account focused on the changes of the 1920s. Regarding sports activities, for example, she offers a succinct but useful description of backgrounds dating from the nineteenth century. By contrast, there is no adequate description of the decline, which had commenced in the nineteenth century, in the role of religion in defining gender relationships and behavior. A brief treatment of this development and of the increasing secularization of society would have been a useful aid to readers unfamiliar with Danish history. Some of the summaries at the ends of chapters approach redundancy and might well have been shortened in favor of such aid.

Nonetheless, Søland has produced an excellent account of the new lifestyles created by young Danish women in the 1920s, and of their role as successful agents of change in rejecting Victorian age behavior in favor of "modernity." She reports that, by the 1930s,

most Danes liked the naturalness and lively energy of young women who no longer hid their physical charms under long skirts and high-necked garments, and who, despite their insistence on greater equality with their spouses, settled into lives as full-time mothers and housewives.

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OLLE LARSSON. *Biskopen visiterar: Den kyrkliga överhetens möte med lokalsamhället 1650–1760* [The Bishop's Visit: The Encounter of Church Authorities with the Local Community, 1650–1760]. (Växjö Stiftshistoriska Sällskap Skrifter, number 7.) Växjö, Sweden: Stiftshistoriska Sällskap. 1999. Pp. 351. 280 KR.

INGER HAMMAR. *Emancipation och religion: Den svenska kvinnorörelsens pionjärer i debatt om kvinnans kallelse ca 1860–1900* [Emancipation and Religion: The Pioneers of the Swedish Movement and the Debate over a Woman's Calling, ca. 1860–1900]. Stockholm: Carlssons. 1999. Pp. 318. 350 KR.

In Sweden, as elsewhere, historians have showed an increasing interest in religion in their analyses of the past. This is especially true regarding the history of mentalities, culture, and popular beliefs in the Middle Ages or early modern period. Religion has, however, also emerged as an important factor in studies of the social history of early modern and modern society. Olle Larsson uses bishops' visitations to study social interaction and religious culture. Larsson has investigated protocols from bishops' visitations in 1652–1656, 1691–1702, and 1750–1759 in Växjö diocese, in southern Sweden. He examines the visitation proceedings, what kind of matters the bishops handled, and how the bishops, local priests, and congregations acted. One of his hypotheses is that the evolution of visitations between 1650 and 1760 reflected developments in the religious culture during this period.

The Lutheran bishops in Sweden regularly visited their dioceses' parishes, as did their Protestant and Catholic counterparts in other countries. From the late seventeenth century, the bishops in Larsson's investigation usually undertook visitation journeys every year. By contrast, the number of parishes visited decreased during the eighteenth century. According to Larsson, ten to fifteen years often passed between a bishop's visitations to the same parish. He concludes that the visitations were probably more occasions for the bishops to supervise local priests and congregations than for the people to meet the church authorities.

A large part of Larsson's thesis, about one third of the book, deals with the visitation proceedings. He gives an exhaustive account of what kind of matters the bishops handled. Since this material has never been thoroughly examined before, Larsson's study is a notable achievement. He finds that matters concerning

the church's economy predominated throughout the era. In the first period, marital matters also loomed large: marriages, betrothals, and sometimes divorces came up during visitations. The number of cases of this type decreased, however, during the later periods. According to Larsson, one can see similar developments in matters concerning order in the parish. However, what Larsson categorizes as "matters about religion" appear to increase at the same time.

Larsson claims that we can observe a transition from episcopal concern for matters of order and morals to matters of religion, among other things. The transition was, as Larsson points out, partly due to an administrative differentiation in the handling of order and moral issues. However, he also maintains that the transition can be viewed as a sign that religion had become a problem for the ecclesiastical authorities during the period. This would, he proposes, indicate that the prevailing religious culture was dissolving during the early eighteenth century, and that the church faced a more individualized perception of Christianity. I think this may be a hasty conclusion as far as it concerns the Swedish countryside during this period. There are also some methodological problems connected with his conclusion. The minutes from the bishops' visitations vary during the period. Those from the middle of the seventeenth century are often quite brief and sometimes incomplete, while minutes from later periods are considerably more substantial. Furthermore, there is reason to question how Larsson categorizes his cases; for example, some matters that he labels as concerning order or morals could rightly be categorized as religious matters.

Larsson seems unaware of international studies in this field. Thus, he has virtually no comparison with the church's appearance in other Protestant or Catholic areas. His analysis of the interaction between bishops and local parishioners would have more merit, if he had also discussed, for example, Keith Luria's study of bishops' visitations in the Grenoble diocese (*Territories of Grace: Cultural Change in the Seventeenth-Century Diocese of Grenoble* [1991]). Like Luria, Larsson asserts that visitations were not merely exercises of control and discipline. The parishioners did not always obey their bishops and sometimes they also raised their own issues at the visitations. With inspiration from Luria's study, Larsson could have made his analysis of those aspects deeper and more complex.

In the late nineteenth century, Lutheran teaching was still an important feature of Swedish culture. Inger Hammar stresses the importance of religion in her book, describing the history of Swedish women and especially earlier studies of women's emancipation as "religion-blind." Religion, the church, and the priesthood are commonly regarded as obstacles to Swedish women's emancipation during the nineteenth century. However, as Hammar remarks, the women who demanded emancipation also had "ideological roots in the very context that research has described as hostile to emancipation" (p. 250). This contradiction has gone

unnoticed, and according to Hammar, nobody has analyzed religion's significance to the early emancipationists in Sweden.

The purpose of Hammar's thesis is to reconstruct and investigate the early Swedish emancipationists' ideology. By doing so, she also wants to show how Christianity could serve as a driving force for the pioneers of emancipation. Her analysis focuses on debates that engaged editors and coworkers of the Swedish journals *Tidskrift för hemmet* (*Home Journal*), published between 1859 and 1885, and *Dagny*, published from 1886. The main character is Sophie Adlersparre (1823–1895), who for many years was both journals' editor and also acted as an influential debater.

In most cases, Adlersparre and her friends debated with representatives of the church and other men who argued for the official, orthodox interpretation of the Lutheran understanding of the relationship between the sexes. According to Martin Luther, a public calling was something reserved to men. A woman's active calling strictly related to the private sphere, where she should function as spouse, mother, and housewife. When Adlersparre and other emancipationists demanded that women should receive education on par with men, they met fierce opposition from orthodox Lutherans who feared that education would lead women away from their "true" calling. They also maintained that women's experience of public life would promote immorality. The emancipationists argued within a strictly Lutheran context. Improved education was, according to them, necessary for women to fulfill their calling as spouses and mothers. Furthermore, they asserted that public morals would actually improve if women, with their God-given chastity, could enter public life.

As a result of different opinions about women's role in public life, Adlersparre's coeditor, Rosalie Olivecrona, resigned in 1869. After that, the journal displayed the influence of religious liberalism more openly. But Adlersparre was far from liberal in the debates concerning morality and sexuality. When other emancipationists in the late nineteenth century proposed new ideas of women's sexuality, she was a stubborn opponent. She also attacked artists and authors whom she considered indecent. When she argued for limiting sex in marriage strictly to reproduction, even orthodox Lutherans opposed her.

Hammar shows that the Lutheran construction of gender was relevant to her subjects. Without knowing the theological context, it is difficult to understand and interpret the debate between the early emancipationists and their opponents in nineteenth-century Sweden. Although Hammar may be unnecessarily polemical in labeling the research as religion-blind, it seems clear that the significance of religion has been underestimated by historians of Swedish women. Hammar's book is an important study that will improve our understanding of the emancipationists.

However, the connection between emancipation and

religion remains unclear in some aspects. Adlersparre and other early Swedish emancipationists certainly were religious and debated within a Lutheran context. The question is to what degree religion was a driving force for the pioneers. One is left with the impression, after reading Hammar's book, that the Swedish Church, the priesthood, and traditional interpretations of the Bible and of Luther's teaching were mighty obstacles to women's emancipation. To overcome them, the early emancipationists had to supplement their Christian outlook with nonreligious nineteenth-century ideas, such as historical-critical thinking and liberalism. Hammar is aware of this, although such factors are not particularly favored in her analysis. Finally, I ought to add that both Hammar and Larsson include lengthy English summaries in their books.

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KLAUS VONDUNG. *The Apocalypse in Germany*. Translated by STEPHAN D. RICKS. Columbia: University of Missouri Press. 2000. Pp. 437. \$39.95.

Klaus Vondung's 1988 study, which has now come out in a good if at times too literal English translation by Stephan D. Ricks, discusses an ancient literary strategy and some of its applications in modern German history and culture. The author, a professor of Germanic and comparative literature, is impressed with the social and political motivations of apocalyptic texts and takes an interdisciplinary approach to his subject. Nevertheless, his emphasis remains on literature. His discussion of the political dimension of the apocalypse, and his treatment of apocalyptic images in the fine arts, are too intermittent for the comprehensive analysis he intended. Instead, he has written a rich and stimulating source book for the historian who wants to pursue the subject further.

The work opens with a conceptual, historical, and linguistic survey of the meaning of the apocalypse in antiquity and its subsequent changes, which provides the basis for later chapters on the nineteenth and twentieth centuries. For the historian, the denial by the apocalypse of the continuity of history deserves particular notice. The Old Testament invested the movement from past to present with the special meaning of progressing toward salvation. But as the history of salvation unfolds, and the righteous are nevertheless defeated by foreign enemies or stray from the faith, the prophets appeal to God to judge the wicked and redeem the persecuted. Their apocalyptic visions foretell the destruction of the old world and the creation of a utopia: the end of history. In the revelation of John in the New Testament, the apocalypse ushers in a "New Jerusalem"—a transition from destruction to fulfillment that, the author points out, "characterized apocalyptic thought to our own century, even when it already distanced itself from its religious origin." Only in the nuclear age, he adds, has the concept shed the

second half of its traditional promise, which once gave meaning to the end of the world (p. 5).

The extensive literature on the subject seems in agreement that apocalyptic formulations emerge from the tension between the "deficiencies" of human existence—frustrations, loss, illness, death—and the longing for their opposites. Already Revelation 21:4 announced "there shall be no more death" in the new world. Some modern versions, the author argues, are also driven by existential terror. As an example, he cites the apocalyptic writings of Ernst Bloch, with Karl Marx one of the theorists who "made substantial contributions to the apocalyptic orientation of German socialism" (p. 5). The transitory nature of life did not lead Bloch to conclude that society, too, was inevitably flawed. On the contrary, the sense of his own mortality spurred him on to develop theories of revolutionary change that resulted in utopia. The author identifies similar motives in the writings of such men as Kurt Hiller and Gustav Landauer (pp. 198–204). But surely not every left or right-wing apocalyptic theorist in modern Germany was driven by fear of dying.

The resort to apocalyptic rhetoric need not be based on the writer's personal situation, but its appeal increases when the community is in crisis. The Babylonian Captivity, the Reformation, the Napoleonic restructuring of Europe, the "deficiencies" of German society in the age of industrialization: all provided fertile ground for apocalyptic visions. The author demonstrates many similarities in the rhetorical strategies of Ernst Moritz Arndt, calling for German liberation from Napoleon; of Marx in *The German Ideology*, among other writings; of German politicians and poets during World War I, who coarsened the apocalyptic utopia into assertions of German superiority; and in the language of Adolf Hitler and other Nazi leaders, whose apocalyptic terminology was not too different from that of any number of expressionistic writers of the Weimar period, even as they aimed for a new end of history in the form of the thousand-year Reich. Nationalism in Germany, Vondung concludes, "was suffused with apocalyptic conceptions from the beginning" (p. 123). His analysis of the Nazis' dichotomy of racial purity and filth, which demanded an apocalyptic resolution, is stimulating. But reliable new insights, and the demonstration of a particular German tendency to think and act in apocalyptic terms, would call for fuller treatment of the Third Reich, and a systematic comparison with other cultures. The challenges of interdisciplinary history can rarely be met by a few forays across disciplinary borders. From the perspective of the historian—or at least of this reviewer—Vondung's study remains incomplete, its wealth of material and sophisticated analysis notwithstanding.

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MARC FORSTER. *Catholic Revival in the Age of the Baroque: Religious Identity in Southwest Germany, 1550–1750*. (New Studies in European History.) New York: Cambridge University Press. 2001. Pp. xiii, 268. \$59.95.

This fine study, following his earlier *The Counter-Reformation in the Villages: Religion and Reform in the Bishopric of Speyer, 1560–1720* (1992), establishes Marc Forster as a preeminent historian of Catholicism in early modern Germany. He now moves south to the politically fragmented and geographically diverse territory of southwest Germany from the Rhine Valley through Upper Swabia, an area where longstanding monastic foundations rose from the landscape, the bishop of Constance represented the chief ecclesiastical authority, and the government of Habsburg Outer Austria was his principal political counterpart. Despite the agricultural character of the region, a notable portion of its population lived in towns. It was the heartland, according to Forster, of the *Reichskirche* and of Baroque Catholicism in Germany from 1650 to 1750.

Forster's purpose is to investigate the origins of the vigorous Catholic identity of the region and thereby "the evolution of Catholicism [in Germany as a whole] by examining the negotiations between different groups about the nature of religious practice and by evaluating the role of intermediaries, particularly the parish clergy, in the development of Baroque Catholicism" (p. 9). Without denying their importance, his book questions the predominant role usually assigned to the reforms of the Council of Trent and to the state in the revival of Catholicism in the Baroque era. He also attributes a greater part to the people: that is, from below, in the form of a lively communalism that survived from the late Middle Ages. As he did in his earlier volume, Forster criticizes the advocates of the confessionalization theory with their heavy emphasis on the disciplining hand of the state in the evolution of the Christian churches. Yet he remains well aware of the difference between southwest Germany and areas with a strong state such as Bavaria. He remarks that, in a sense, for the area under study the whole period of the Reformation, Counter Reformation, and Catholic Reform with its efforts to simplify and rationalize religious practice might be considered an interruption in the evolution of popular religiosity from the late Middle Ages to its peak in the mid-eighteenth century.

Forster's first chapter deals with the period of the Counter Reformation and Tridentine Reform, which he dates from 1550 to 1650. The era enjoyed its successes, such as the elimination of concubinage among the parish clergy, but most of its initiatives dissipated or were brought to an end by the Thirty Years' War. Only after 1650 did there begin to emerge the distinctive Catholic identity of the Baroque era. "Decisive . . . was the willingness of the Church to tolerate and even support popular religious practices, and the simultaneous popular appropriation of many

clerical initiatives" (p. 106). The next two chapters describe sympathetically features of Baroque religious practice: the sacral landscape of churches, monasteries, shrines and associated pilgrimages; sacraments of passage and observances of the liturgical year; and the emergence of individual devotion after about 1700. Two "interlocking aspects" (p. 154) of Catholicism in southwest Germany are the topics of chapters four and five: clericalism and communalism. By popular clericalism Forster understands an esteem for the clergy, recognition of the importance of their role, and the conviction that they were servants of the community. Communalism dated from the late Middle Ages and designated communal control of the parish and oversight of the pastor's conduct through village communes or town councils. Although there certainly were conflicts between pastors and their parishioners, general harmony between them characterized Baroque Catholicism. In his final chapter, Forster discusses those who served as "intermediaries" (p. 228) between the demands of the people and the requirements of the church. These were the Jesuits and Capuchins, the great monasteries of the area, but above all the parish priests, upon whose role Forster focuses throughout. In them converged the Tridentine pastoral ideal and the communal tradition of the pastor.

At the end, Forster finds an affinity between the communal character of Baroque Catholicism in southwest Germany and the communalism located by Peter Blickle in Switzerland and south Germany in the late medieval and Reformation periods, and just as Blickle discovers here a building block of the democratic tradition in Germany, so Forster suggests the same for the Catholic communalism of the Baroque era.

Forster has little to say directly about the moral conduct of the people. This is an important issue for advocates of the theory of confessionalization, who generally claim that overall moral improvement contributed to later modernization. Was there a change between 1550 and 1750 in southwest Germany in the direction of industriousness, punctuality, less inclination to excess in food, drink, and sex, and greater charity, and if so or if not so, what was the significance for Baroque Catholicism?

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PAMELA E. SELWYN. *Everyday Life in the German Book Trade: Friedrich Nicolai as Bookseller and Publisher in the Age of Enlightenment 1740–1810*. (Penn State Series in the History of the Book.) University Park: Pennsylvania State University Press. 2000. Pp. xvi, 419. \$75.00.

Friedrich Nicolai—bookseller, publisher, author, and *Aufklärer*—would have approved of this book. Like many of the volumes he published and sold in a career spanning the rise of a self-conscious modern German public, it enlightens and informs, eschewing the latest

philosophical and literary fancies for sober, solid, and comprehensive analysis. Pamela E. Selwyn has digested an enormous amount of archival material, including a significant proportion of letters in the Nicolai *Nachlass*, to provide an in-depth look at the German book trade in the second half of the eighteenth century, seen through the prism of one of the most protean figures of the era.

Neither a biography nor an intellectual history of Nicolai and his times, the book is rather a history of a major Enlightenment figure, and the process of enlightenment itself, from the bottom up: paper, bindings, bookshop employees, contracts, censorship, book fairs, catalogues, editing, subscriptions, shipping, translations, and so on are the raw materials of this book, as they were of the book trade generally. Although the detail can become a bit overwhelming at times, the narrative eventually produces an image of Nicolai and the German book trade that is more than the sum of its parts. As such the book usefully complements Horst Möller's intellectual biography of Nicolai, *Aufklärung in Preußen: Der Verleger Publizist und Geschichtsschreiber Friedrich Nicolai* (1974).

Following an introductory chapter is a chapter providing a nuts and bolts account of Nicolai's publishing activities, which began during the Seven Years' War and continued to the Napoleonic occupation of the Germanies. We learn about everything from the difficulties of acquiring good paper to how manuscripts were reviewed to the quantities of books published by genre, language, and topic. Pedagogical works and school books were central elements of Nicolai's publishing program from the beginning; such works sold well while advancing Nicolai's down-to-earth conception of enlightenment: providing basic information of all sorts to general readers in an idiom they could understand. Indeed, a pervasive theme that emerges from the book is the degree to which Nicolai functioned as an intermediary between the wider Enlightenment republic of letters and a nascent German public of readers.

Chapter three, "Everyday Life in the German Book Trade," examines the German book trade in depth and includes analysis of the all-important biannual book fairs, the foreign book trade, the actual locations of Nicolai's shops over the years, and the way in which booksellers like Nicolai often functioned as procurers of books. Catherine the Great, for example, had a standing order for all the historical works in German and French on all the countries of the world, a task that took Nicolai a number of years to complete.

Chapter four examines "The Legal and Political Framework of the Eighteenth-Century Book Trade," providing extensive analysis of laws regulating the book trade, piracy, censorship, and authorship as they evolved over the second half of the century. It becomes clear that censorship, "a time-consuming and at times depressing part of [Nicolai's] everyday life" (p. 190), was basically self-censorship by the intellectual elite. Censors were often friends and colleagues of Nicolai,

and there was a good deal of negotiation with royal officials along the way. Readers are also likely to gain insights into the complexities surrounding book piracy (*Nachdruck*) in an era when authors had virtually no rights and publishers relied on specific privileges to protect particularly attractive books from unauthorized reprinting.

Chapter five presents an extended analysis of Nicolai's star publication, the *Allgemeine Deutsche Bibliothek* (*ADB*): its genesis, function, the mechanics of publishing such a mammoth review journal, problems with censorship, and its eventual decline as other currents in German letters came to the fore. During its heyday, the *ADB* was both loved and feared as preeminent arbiter of German letters, leading in one case to a pastor denouncing a negative review of a book of his sermons from the pulpit. Chapter six details Nicolai's relationship with his authors, documenting the degree to which idealism and the profit-motive were intertwined in Nicolai's publishing career.

Selwyn conclusively demonstrates that seen from the bottom up, the German Enlightenment was more about fostering and educating an emergent "public" than advancing some kind of disembodied and oppressive rationality, and it is satisfying to see a neglected and often unfairly criticized figure like Nicolai receive the kind of sustained and comprehensive treatment that he clearly deserves.

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ELISABETH MANTL. *Heirat als Privileg: Obrigkeitliche Heiratsbeschränkungen in Tirol und Vorarlberg 1820 bis 1920*. (Sozial- und Wirtschaftshistorische Studien, number 23). Vienna: Verlag für Geschichte und Politik, distributed by R. Oldenbourg, Munich. 1997. Pp. 262. DM 80.

Post-Napoleonic Europe witnessed the selective reimposition of marriage controls, specifically in a number of South German states and Austria-Hungary. In the case of the Tyrol, the provincial government introduced legislation that required formal approval of marriage in 1818–1820 (the so-called *Ehekonsens*) for all domestic servants, journeymen, day laborers, and lodgers. In 1850, amid fears of further population growth and mass poverty, the authorities reinforced the existing legislation, which was now to be applied to virtually all individuals intending to marry. Despite the abolition of *Ehekonsens* in the Austrian crown lands in 1869, official restrictions on marriage in the Tyrol were not lifted until January 1921. The issue of marriage controls in nineteenth-century Europe has attracted attention from a number of historical anthropologists, as well as from demographic and family historians, but the case of the Tyrol is particularly interesting because of the persistence of low nuptiality and low rates of illegitimacy, and the obvious contrast between the severity of marriage controls and the limited threat of rapid population growth.

Elisabeth Mantl has assembled an impressive range of sources in order to analyze the complexity of the marriage process and the extent to which marriage controls essentially reflected economic and social factors. The data base utilized approximately 6,000 marriages between 1850 and 1900, each of which involved a formal application to the parish authorities for permission to marry. It also included individual appeals against the refusal of the *Ehekonsens*, as well as formal objections by the parish authorities to the granting of permission to marry on appeal by the district administration (*Bezirksamt*). The analysis focuses on three specific parishes (two rural and one urban), and Mantl maintains a useful comparison between the situation in the Tyrol, as a relatively backward agricultural region, and the Vorarlberg which underwent significant economic development in the course of the nineteenth century.

This is an important study that effectively combines both qualitative and quantitative analysis. The imposition of marriage controls in the Tyrol was designed primarily as a means of strengthening the existing social structure rather than regulating population growth. In reality, those unable to provide evidence of an independent existence were excluded from marriage and in certain cases, as in Kitzbühel, over fifty percent of applications for *Ehekonsens* were rejected. Even stricter controls were imposed on outmigrants wishing to return and marry, as well as on "foreign" brides. As a result, the possibility of marriage was increasingly restricted to indigenous individuals of independent means whose overall behavior was deemed to be satisfactory. But the imposition and extension of marriage controls was a reaction to an underlying fear that a rise in nuptiality among the propertyless lower class would threaten the structure of rural society. Despite increasing demands from the central government for a liberalization of marriage restrictions, the parish authorities in the Tyrol rigorously maintained the requirement that all couples seek official permission for marriage. It had its origins in a rural society already characterized by substantial inequality, and it continued to provide village elites with an effective barrier against radical change. The practice of *Ehekonsens* was maintained in the Tyrol precisely because it institutionalized existing forms of social control and reaffirmed the traditional social hierarchy. By contrast, the consequences of such a policy for a large number of individuals were substantial: the majority of servants and journeymen were simply forced to renounce any prospect of marriage.

By reconstructing the process by which individual applications for *Ehekonsens* were dealt with by the various authorities and by focusing on a significant number of microhistories, Mantl has produced an impressive contribution to the debate over the origins and significance of marriage controls in the nineteenth century. Perhaps inevitably, there are some areas where the interpretative strength of the analysis could have been reinforced. Although Mantl recognizes the

importance of income-earning opportunities for women, particularly in the informal sector, in determining the economic viability of marriage, the data preclude a more detailed analysis of trends in female labor force participation, whether in cigar production or in the clothing and textile trades. No attempt is made, even on a counterfactual basis, to assess whether the Tyrol could have absorbed the additional population pressure that a more relaxed attitude to marriage controls may well have generated, and insufficient attention is devoted to assessing the impact on rural communities of changes in the extent of both in and out-migration on marriage propensity. The quality of the individual-level analysis is to be commended, but a more detailed reconstruction of the dynamics of social interaction, both at the family and village level, would have been invaluable. Although the imposition of marriage restrictions was a key mechanism in sustaining social inequality, it would have been useful if the analysis could have focused on the role of regulatory controls on migration and house construction, which were also important in reinforcing the traditional structure of local society. But these points should not detract from the overriding qualities of a study that represents an important contribution to our understanding of the significance and impact of official controls on marriage that persisted in the case of the Tyrol, into the twentieth century.

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ULRIKE KIRCHBERGER. *Aspekte deutsch-britischer Expansion: Die Überseeinteressen der deutschen Migranten in Großbritannien in der Mitte des 19. Jahrhunderts.* (Beiträge zur kolonial- und Überseegeschichte, number 73.) Stuttgart: Franz Steiner. 1999. Pp. 508. DM 166.00.

Most Germans by 1914 believed that their country had been denied its proper "place in the sun" by the selfish policy of rival powers, particularly Britain. They looked out and saw how small was the German overseas territory as compared with that of the great maritime power, or even worse, the enemy France. Pressure groups had been formed to aid Germany's expansion: the Navy League to build a navy equal to that of Britain's, the Colonial League to acquire more colonies, and Pan-Germanism to promote German cultural influence outside of Germany. Much German national resentment directed against Britain grew out of a mixture of envy and hate that focused on the superiority of the upper classes. Thus, many a German tourist, perhaps or perhaps not accidentally, given an Italian or Spanish hotel room inferior to that given to a traveling Englishman, came home burning with indignation that Germany, and now Germans, were being denied their "place in the sun." Unfortunately, many Germans soon came to regard it as necessary or even desirable that Germany be recognized as not only equal but culturally superior to all others. No one

symbolized this conflation of envy and hate toward the English better than Kaiser Wilhelm II, who had assumed power in 1888 and dismissed Otto von Bismarck and his cautious policy of defense. Wilhelm II loved to portray himself as the brash, Prussian warlord. He was rarely photographed out of uniform, always appearing unsmiling, resolute, and utterly disdainful of all—but particularly the English. Yet this was all a charade. Much of the kaiser's posturing was specifically designed to hide the fact that his left arm was crippled and almost completely useless. Wilhelm, symbol of the new dynamic Germany, projected the image of utter contempt for the English yet hid a sense of inferiority and envy in the face of England's institutions, empire, and navy.

Ulrike Kirchberger seeks to discover how all this might have happened in her book, originally a Ph.D. dissertation at the University of Stuttgart. Focusing on the 1850s and 1860s rather than the end of the century, when Pan-Germanism and colonial expansion were the norm in imperial Germany, she reveals the complex relationships that German immigrants to Great Britain had with British institutions, customs, laws, and particularly the empire. Many who came to Great Britain were fleeing the repression imposed by German states after the failed revolutions of 1848, with Karl Marx and Friedrich Engels being now most remembered. Many German liberals saw the political system and freedoms of Great Britain as a beacon of hope for their own attempt to bring about a liberal unification of Germany. A large proportion of the Germans who came to Britain used it as a brief stopover before going on to the United States, Canada, or Australia. Kirchberger details how German organizations were established to assure that a measure of German culture could be preserved and honored, but this proved difficult because of the ease of assimilation in British society. Schiller festivals in Britain did propagate the idea of a German community abroad, but there was a close cooperation between German and British gymnastic societies. The German gymnasium was used by the Scotch Rifle Volunteers, and German athletes participated at the festivals of the British London Gymnastic Society. Germans participated at every level of the British colonial enterprise acting as explorers, scientists, missionaries, orientalist, and entrepreneurs. Another influential circle of Germans was made up of the artists, musicians, librarians, and scholars who were associated with Prince Albert. Many different groups of Germans came to Britain for a variety of reasons, but, as Kirchberger points out, no cohesive community with a firm social structure developed.

Although there may not have been a cohesive German community within Britain, the German National League in Britain, an organization set up to promote German culture, did transform its nationalism from a narrow emphasis on unification in the 1850s to an idea of a German *Weltreich* and a strong German navy. It all started out innocently enough with the idea that, in order to preserve the ethnic identity of Germans—too

easily prone to assimilation in Britain—overseas settlements should be established. Quite simply put, colonialism became important and an nationalistic option for the National League in Britain because questions of overseas expansion were so much part of the political debate in that country. In fact, the migration of important imperialist pressure groups to Britain and individuals like Lothar Bucher and Max Schlesinger helps to explain the lack of interest in overseas expansion in the 1850s and 1860s within Germany itself. What, then, is the continuity that exists between the German National League in Britain and Kaiser Wilhelm II and the Pan-German League? While the ideas of expansion held by the National League were tempered by the liberal ideas of the forty-eighters, its members shared with the later imperial pressure groups the sense of bringing light to “dark continents” and encouraging German emigration to overseas settlements. The National League’s doctrine of a worldwide ethnic community grew out of the unique circumstances involving German migrants to Britain, who both admired and feared the attractions of British institutions and who saw in the model of British colonialism an opportunity to preserve “Deutschtum im Ausland.” What had begun as an attempt to stop German assimilation into British society evolved into a dangerous image of the global hegemony of the German race, something that would have a bloody future.

Although a great deal has been written about Germans in Britain, this study does add much to our understanding of German perceptions of Britain and the empire in the nineteenth century, German colonial history, and, more specifically, how an nationalist ideology imported from Germany was transformed by ideologies and influences from the new country. Thus one goes from Prince Albert to Kaiser Wilhelm II.

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RUDY KOSHAR. *German Travel Cultures*. (Leisure, Consumption and Culture.) New York: Berg. 2000. Pp. x, 241.

In this book, Rudy Koshar seeks to shed light on the evolution of modern tourism, using a selection of travel guidebooks to Germany as his primary source. The monograph, one in a series of which Koshar is general editor, also has a more ambitious aim: “to ask how (or if) leisure practices [such as tourism] gave rise to enduringly important conceptions of identity and the self in modernity” (p. ix). Chapter one focuses on the Baedeker guide, said to be the “Bible of tourism . . . a synonym for leisure travel itself” (p. 65). Taking his story back to the mid-nineteenth century, when the first Baedekers appeared, Koshar draws on various editions of the Baedeker guidebooks devoted to Germany’s regions and, from 1906, to the country as a whole, to analyze the “national-liberal travel culture.” According to Koshar, this culture “rested on the

metaphorical figure of the collector/traveler,” for whom traveling, although “an important source of collective identity,” had a “deeply personal character.” Rather than being forced to engage often costly professional guides or to submerge his identity (Koshar tells us that the nineteenth-century German traveler was almost invariably male) in a commercially oriented group, the individual traveler was freed by his Baedeker to make his own choices and seek adventure for himself (pp. 29–31).

In succeeding chapters, Koshar examines four other “travel cultures.” Chapter two, on “Sex and Class,” explores the working-class travel culture that emerged in the 1920s and 1930s, using several inexpensive guidebooks addressed to economy travelers (including women) and a 1932 guide addressed specifically to socialist workers. He also introduces Berlin’s night life by way of a 1931 guide to that city’s sexual topography and looks at several guidebooks that catered to U.S. travelers who flocked to Germany in increasing numbers during the Weimar years. Chapter three, “Savage Tourism,” deals with the National Socialist period. In this chapter, perhaps the most intriguing in the book, we learn about a guide for SA members that points them to the sites in Berlin associated with that Nazi paramilitary formation’s struggle against the Communists in the last days of the Weimar Republic. Among other things, Koshar also gives us a critical account of the Nazi regime’s “Strength through Joy” vacation travel offerings and an analysis (based on contemporary guidebooks, including the 1943 Baedeker) of wartime leisure travel by Germans within their country and in the conquered territories.

The book’s final chapter, “Fodor’s Germany,” takes us through the first postwar decade that saw Germany divided by the Iron Curtain. Started by a former Hungarian, the Fodor guide was geared to the U.S. traveler who came to explore a West Germany that was rapidly rebuilding after Germany’s devastating defeat in World War II. Koshar uses Fodor to analyze the emerging “American-German travel culture” that, he claims, replaced both Baedeker’s “nationally oriented liberalism” and “the interwar theologies of sex, class, and race” (p. 202).

Koshar’s topic is an intriguing one, and his monograph—based on a wide array of primary and secondary sources—undoubtedly represents a serious contribution to the burgeoning literature on modern tourism. But unless the reader is armed with a good dictionary (imbrication? sessility?) and schooled in the convoluted language that is increasingly employed in contemporary discourse on cultural and sociological topics, he/she will find the book hard to get through. Only the specialist is likely to find the effort worthwhile.

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ALFRED C. MIERZEJEWSKI. *The Most Valuable Asset of the Reich: A History of the German National Railway*.

Volume 1, 1920–1932. Chapel Hill: University of North Carolina Press. 1999. Pp. xx, 482. \$75.00.

ALFRED C. MIERZEJEWSKI. *The Most Valuable Asset of the Reich: A History of the German National Railway*. Volume 2, 1933–1945. Chapel Hill: University of North Carolina Press. 2000. Pp. xxi, 248. \$45.00.

At first glance, this work seems misproportioned. The first volume, dealing with Germany's railways in the Weimar era, is twice as long as the volume covering the Third Reich. Given the importance of the railroads for Germany's World War II effort, the imbalance is striking, even when Alfred C. Mierzejewski's earlier and excellent study of the rail system's final collapse in 1944–1945 is taken into account. Yet appearances are deceiving. The respective lengths of the volumes have nothing to do with depth of coverage. Rather, they reflect Mierzejewski's convincing thesis that the revival of Germany's railway system after World War I, and its reconfiguration as a national institution, established not only the parameters but in many ways the details of its functioning in the Nazi era.

Germany had gone to war in 1914 with a railway system whose organization reflected the Second Empire's federal nature. Among the first goals of the new government was not merely to remedy the consequences of four years of hard service and neglected maintenance, but to create a genuinely national system comprehensively serving every corner of Germany while meeting and matching regional and local needs.

Railway managers, heirs to a comprehensive legacy of *dirigiste* economics, defined the railroad as a community enterprise. Profit-making in general, and the specific effectiveness of particular aspects of the system, took second place to serving the public directly, by providing generally available transportation and indirectly, by binding together a Reich whose unity, after 1918, seemed too fragile for comfort. Mierzejewski highlights the role of Transport Minister Wilhelm Groener, late of the imperial army, in developing the railroads' commonwealth aspect in the early 1920s. During the great inflation he overbought locomotives for the sake of heavy industry. He maintained a work force larger than needed in the interest of full employment. The long-term consequences for the Reichsbahn, including distorted procurement policies, massive pension bills and the public opprobrium that accompanied the periodic dismissal of personnel without functions, were considered acceptable tradeoffs.

German policies were challenged from the beginning by Allied demands that the postwar railways be organized along market-oriented, profit-focused lines. This involved more than a perception of the railroad system as an obvious source of reparations. Both French and English advocates of the new approach were principled believers in private capitalism as the best solution to Germany's postwar economic malaise. The German National Railway, created in April 1920, became the largest enterprise in the capitalist world during the 1920s. It was a correspondingly logical

long-term source of repayment for the Dawes Plan reparations loans in 1924, and was accordingly reorganized from a state to a company-operated institution: from the Deutsche Reichsbahn to the Deutsche Reichsbahn Gesellschaft.

The company's board of directors, chaired by Carl Friedrich von Siemens, agreed that the Reichsbahn was still primarily obligated to serve the public good. It was best able to do that, however, if run like an efficient private concern. The Reichsbahn improved its internal efficiency, especially its accounting systems. The new system combined centralized management with decentralized operations and customer service, successfully meeting the increasing challenges posed by the internal-combustion engine. Motor buses became a significant part of the Reichsbahn, not only supplementing or replacing trains but also extending service to areas where railway construction was unfeasible.

Even that relatively modest market model, consistently was challenged. Mierzejewski makes clear that dislike of the new system was more than rejection of its connections with Versailles and reparations. The Deutsche Reichsbahn Gesellschaft remained a foreign body because it was seen as fostering the values of a free market that denied public responsibility.

Mierzejewski makes a convincing argument that the reorganized railroad system was in fact capable of paying reparations without damaging either its own operations or the German economy. To do so, however, it would have to improve its internal efficiency even further, clearly privileging market forces over social utility. The Reichsbahn was expected—and expected itself—to move goods and people quickly, cheaply, and regularly. Earning profits of any kind, much less profits to pay reparations, was far down the list of priorities, at least in principle.

Both public and private financial management in Germany had a tradition of preserving the firm and its jobs at the expense of growth, or of paying dividends. Railway administrators saw themselves as civil servants, not businessmen. They considered themselves morally superior because they were not impelled by something as base as individual or institutional profit—a fundamental difference that to this day influences relations between the German and the U.S. governments, particularly when Republicans are in office.

Responding to a growing hostility to private enterprise that comprehensively permeated the Weimar system, the Reichsbahn heavily funded projects that were justifiable in social or cultural terms, but less relevant to the real needs of transportation and business. During the depression, Heinrich Brüning's government in particular compelled the Reichsbahn to hire people and purchase rolling stock, and to initiate construction projects it did not need, for the sake of the commonwealth.

Efficient, effective, yet to a degree alienated from the society it served, the Reichsbahn might have been

expected to welcome the Third Reich, with its avowed ethic of "public welfare over private good." Being able to abandon the profit system did prove gratifying to Reichsbahn officials. Mierzejewski, however, stresses the narrowly focused, bureaucratic nature of the railways' reaction to the Nazi New Order. The Reichsbahn responded to the creation of the Third Reich along bureaucratic lines. It wholeheartedly supported state policy because that was what state institutions did. It retained a central role in providing transportation service. It neither asked questions nor made demands beyond those of blinkered routine. Reichsbahn funds were diverted without accountability. Reichsbahn rolling stock was put at the service of the Holocaust. Reichsbahn officials misused material and human resources in occupied regions. In each case, the railroads' behavior was shaped less by specific affirmation than by inwardly focused indifference.

What emerges from Mierzejewski's second volume is an institution lobotomized by standard operating procedures, indifferent to the point of solipsism to anything beyond schedules, maintenance, and procurement. By Third Reich standards, even the Reichsbahn's institutional infighting over issues such as control of the autobahns was half-hearted. Given the current emphasis on the positive involvement of Germans and German institutions in the Third Reich, Mierzejewski's characterization seems almost old-fashioned. The Reichsbahn emerges as an embodiment of the "German Michel:" at bottom, too unimaginative to be really malevolent. The consequences for Germany and Europe were, however, no less catastrophic.

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MICHAEL H. KATER. *Composers of the Nazi Era: Eight Portraits*. New York: Oxford University Press. 2000. Pp. xiii, 399.

Michael H. Kater is well known for his study of music and musicians in Nazi Germany: jazz in 1992, classical music in 1997 and, now, eight composers: Werner Egk, Paul Hindemith, Kurt Weill, Karl Amadeus Hartmann, Carl Orff, Hans Pfitzner, Arnold Schoenberg, and Richard Strauss. The text again is learned, grounded in impressive research. Inevitably quoting himself throughout the work, Kater draws on the private papers of the composers and testimony of intimates whom he interviewed. Due to the rivalries between, say, the friends Orff and Egk, as among modernist refugees Schoenberg, Weill, and Hindemith, we learn of private views, nasty campaigns, and who slept with whom. Neither Nazi collaborators nor their victims constituted groups of solidarity. Vindictive gossip was common to all and is recorded by the historian.

Kater continues a tradition, begun during the Nazi period by early emigrés, of revealing the collaboration of musicians with the Nazi regime's cultural policies.

Scholarship has been divided between the work of those who have focused on the careers of musicians who tried to ply their trade regardless of political context (scholars regarded as apologists by their detractors) and those, like Kater, who want to set the record straight about the service rendered by musicians toward a cultural facade for the terror state. The heated debate has focused on the gray world of distinguished non-Nazi musicians who remained and worked in Nazi Germany.

Armed with new and old information Kater proceeds as a prosecutor, which is easy in the case of Pfitzner, a late-Romantic composer whose chauvinist rhetoric long before the Third Reich helped define Nazi cultural policy. His complicity is self-evident, even though Kater's generally judgmental tone is replaced by a relatively sympathetic one due to the difficulties the cranky and self-aggrandizing musician experienced in Nazi Germany. Collaboration is also obvious in the case of the most important composer among those who remained in Nazi Germany, Strauss. The first president of the Reich Music Chamber until his forced resignation in 1935 (a major scandal) and the subject of intense controversy in a considerable literature since the war, Strauss clearly was no Nazi. Selectively he kept his distance, withheld support and demonstrated opposition, and was protective of his Jewish daughter-in-law and "half-Jewish" grandchildren, whom he loved dearly. Yet Strauss contributed to the musical facade of the Third Reich simply by his presence and musical work, cultivating connections, accepting honors, special privileges, and positions. In his lengthiest and most even-handed portrait, "Jupiter Compromised," Kater surprisingly exonerates the grand old man of music in Nazi Germany.

Kater's prosecutorial style is most evident in the case of younger careerists. The composer Egk stands for the accommodation of creative people in Nazi Germany and questions about the degree of collaboration (or of opposition). Influenced by modernist trends in music (especially Igor Stravinsky) before 1933, Egk continued to compose and propagate "new music"—albeit within the standards acceptable to policy shapers such as propaganda minister Josef Goebbels, although offensive to rigid party ideologists gathered around Alfred Rosenberg. The characteristic division within the Nazi leadership and hierarchy is thus illustrated. Egk's works were performed and earned him high salaries, prizes, and official honors. He rose to lead the organization of composers within the official Reich Music Chamber. Labeled "the enigmatic opportunist," Kater traces Egk's successful career through the Nazi period and beyond. Some of the most valuable and, in American literature, least well-known information covers Egk's denazification after the war and the continuation of a successful career made possible by the survival of the "old-boy network" from one regime to the next. Egk's friend Orff, originally identified with anti-Nazi musical circles, became a successful and honored "fellow traveler"

after the triumphal premiere of *Carmina Burana* in 1937, according to Kater, participating in a Nazi-sponsored competition to rewrite Felix Mendelssohn's famous *Midsummer Night's Dream* music and other compromising activities. With the help of a member of the American military (a former student of his), Orff managed to clean up his image during his denazification proceedings. Judgment regarding Orff today remains mixed; Kater judges "the man of legend" most severely.

Kater's critical tone is even extended to obvious victims. The refugee Hindemith enjoyed a considerable reputation as a modernist before 1933—a negative one in Nazi eyes—yet Kater points out that he tried to stay in Germany and accommodate. After the Nazis rejected him, he became the "reluctant emigre" who, after the war, was the "reluctant remigrant." The Jewish Weill and Schoenberg, denied the possibility of an arrangement, also expressed "German" sentiment in arguments with foreigners. Even in the case of the "dissident" Hartmann, who stayed in Germany and wrote music inspired by anti-Nazi sentiment, the prosecutorial Kater wonders why the bitter composer did not leave Germany and how he managed to travel to international music festivals abroad, hinting at the possibility of compromise with the regime.

In his eagerness to convict, Kater sometimes stretches the evidence. He likens the language of Egk to that of the SS and Auschwitz (p. 17) and refers to his collaboration as "all the more despicable" for him being a non-Nazi (p. 21). Strauss's illegitimacy is compared to that of Hitler in its psychological consequences (p. 213). The "notorious gambler and philanthropist Otto von Pasetti," writes Kater, "had an affair with the 'nymphomaniac' Lotte Lenya, Weill's wife (pp. 74, 270). The 'narcissistic' Orff had 'an obsession with sex . . . he had no fewer than four wives' (p. 141). Kater hits especially hard at powerful conductors who were attractive to women, including opponents of the Nazis like the socialist emigré promoter of "new music," Hermann Scherchen, an "egomaniac and consummate womanizer" (p. 100). Kater has argued in the past against the "womanizing" of Wilhelm Furtwängler. Not accepting the great conductor's documented acts of defiance as proof of genuine resistance, Kater now adds to sexual excess other negative character traits including vanity, hypocrisy, venality, and authoritarianism. Finally, most disturbingly, he writes that Furtwängler "had an almost pathological tendency to identify with and protect its victims, Jews, non-Jews, and sometimes even National Socialists" (p. 38). What is one to make of this psychologizing of a person earlier identified as a collaborator by Kater and other detractors?

On balance, Kater emerges in some cases as fair judge, in others as tough prosecutor, as he reflects in himself the division within the community of those

who continue to be fascinated by this intriguing subject.

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JAN-WERNER MÜLLER. *Another Country: German Intellectuals, Unification and National Identity*. New Haven: Yale University Press. 2000. Pp. 310. \$27.50.

In November 1989, TV screens and the front pages of newspapers were filled with pictures of East Germans streaming through, around, and over the Berlin Wall on their way to the West. If this symbol of the Cold War could so easily be traversed, then perhaps that ideological battle that split the world in twain for over forty years was now officially over. A year later, the twain had met; Germans—East and West—were unified into a single state. However, if in Germany the Cold War no longer existed, no one was willing to predict with any certainty what shape a new Germany would take. Would a powerful nation in the middle of Europe forget the lessons of its troubled past as it charged ahead to define its future? West German intellectuals, who had come of age, established their reputations, and gained international recognition in a divided Germany, asked themselves the same questions. They now found themselves living in another country, the title of Jan-Werner Müller's account of German intellectuals, unification, and national identity.

Intellectuals, Müller tells us, play a key role in shaping public opinion in Germany. Their thoughts are not tucked away in the Op-Ed part of the newspaper; rather, they are on page one. They publish not only in journals aimed at a small, highly educated elite but also in *Der Spiegel*, the popular weekly that is an upscale German version of *Time* or *Newsweek*. They speak, and Germans listen. The intellectuals of greatest concern to Müller are the writers Günter Grass and Michael Walser, the philosopher Jürgen Habermas, and the literary theorist and editor of a major journal, *Merkur*, Karl Heinz Bohrer, "one of the most influential intellectuals in post-unification Germany" (p. 177) who came into public view in the late 1960s, about a decade after the others at the center of Müller's study. Many others—Hans Magnus Enzensberger, Ralf Dahrendorf, and representatives of the Left of 1968—appear as well. Walser and Grass were both born in 1927, Habermas only two years later. This makes them representatives of the *Flakhelfergeneration*—those old enough to have been shaped by their experiences in World War II but too young to have reached political consciousness. Writing in the 1950s, the sociologist Helmut Schelsky saw in these young Germans a "sceptical generation," uninterested in politics and seeking solace in postwar economic prosperity. In contrast, Müller finds in this cohort intellectuals who have consistently been committed to overcoming the historic reluctance of German intellectuals to become

involved in the sordid business of politics. They defined themselves explicitly in opposition to the image of the “mandarin” of the Weimar republic, a “*Geistesaristokratie* [intellectual aristocracy]” (p. 11), and they faulted Weimar intellectuals for standing by as Germany’s first experiment with democracy failed and acquiescing all too readily to the lure of National Socialism. For this group, intellectuals who came of age in the late 1950s, political engagement was a responsibility, not something to be avoided at all costs.

The intellectuals Müller discusses reached very different conclusions about the consequences of unification. Grass vociferously opposed unification, arguing for the integrity of a distinctive East German identity that embodied not only forty years of dictatorship but also the triumphant revolutionary overthrow of an oppressive regime. Behind his conception of a confederation was also the fear that a unified Germany would inevitably become a power in Central Europe that would arouse the fears of its neighbors and reproduce the aggressive patterns of its past. Habermas was no less critical of what he called the West German “*Anschluss* [annexation]” (p. 99) of the East, but he saw ways to avoid repeating the past in “unconstrained communication” and an invigorated public sphere that would provide the proper antidote to “DM Nationalism” (p. 108) and a national identity scripted by economic and political elites, not the people. Walser, whose idiosyncratic political journey had led him from the communist left to outspoken nationalism, celebrated unification and emerged as the counterpoint to Grass, the pessimistic naysayer of 1990. For Bohrer, unification held the potential of Germany’s escape from a narrow-minded provincialism and attainment of a “modern (and modernist) culture” (p. 186). The rewards far exceeded the risks. And the left—the heirs of the politics of 1968—viewed unification with a certain melancholy, mourning the loss of the utopian vision of a communist alternative that, if flawed, now belonged only to history, not the realm of imagination and the possible.

The strength of Müller’s analysis is that he locates this range of views in the longer-term trajectory of West German intellectual, political, and social development since 1945. What intellectuals said in the immediate aftermath of unification was of a piece with what they thought of the nation, political institutions, and democratic politics before the wall came down. Even Walser’s twisted path makes sense when set in the larger context that Müller provides. Joining the intellectuals discussed here is a common preoccupation with the past of National Socialism and the Holocaust. How they view unification is inextricably linked to their assessment of how Germans can most effectively realize what Karl Jaspers and Theodor Adorno called for in the late 1940s and 1950s: a self-conscious, sober confrontation with the multiple legacies of National Socialism. In important respects, post-1990 Germany is another country, but this insistence on maintaining Auschwitz as a point of reference

defines a continuity that unification redefined but did not rupture.

“The intellectual field,” according to Müller, “is populated by a number of writers, academics, and, to some extent, prominent journalists” (p. 13). Who’s in and who’s out are, however, not always clear. While some of Müller’s choices are obvious, some of his exclusions are not. A fuller delineation of the category of intellectual might help to make sense of the selection criteria. For example, Bohrer, one Bielefeld intellectual, is crucial, but the historian Jürgen Kocka, a former Bielefelder now in Berlin, who played a major postunification role in shaping a research agenda for the study of East German history, is not. “[I]n Germany,” Müller tells us, “unlike in Britain and the United States, it is almost self-evidently legitimate that men and women who have distinguished themselves in cultural and academic matters, should comment on affairs of state” (p. 14). But in this book, the “contestants on the ‘intellectual field’” (p. 13) all seem to be male. The Green politician Antje Vollmer briefly appears, but she is the exception that proves the rule. The German public sphere in which intellectuals function may include no women, but if this is the case, this absence should be explained. East Germans are also not present, but Müller tells us that the censorship of the public sphere in the German Democratic Republic would prevent any systematic pre-1989 East-West comparisons. However, other transplanted East Germans—say, Wolf Biermann, who witnessed the move toward unification from the West after involuntarily leaving the East and who has much to say about unification—might have leavened the mix of intellectuals whom Müller presents.

More than a decade after unification, the shape of the Berlin republic is still hotly debated. Should Germans be sending troops into combat as part of NATO forces? Should Germany change its immigration and citizenship laws and embrace some variant of multiculturalism? Can it be a “normal” nation and acknowledge Auschwitz as a central part of its past? Müller provides reassurances that the most influential answers will not come from the New Right, whose attempts to reject “hedonistic liberalism” (p. 203) in the early 1990s found no significant audience. Rather, the intellectuals who will have something to say “of real consequence for these real problems” (p. 2) are those on the left and in the middle who are at the center of this study. Müller’s important book gives us a useful roadmap for following the route that German intellectuals have taken in their quest for solutions.

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JEFFREY R. WATT. *Choosing Death: Suicide and Calvinism in Early Modern Geneva*. (Seventeenth Century Essays and Studies.) Kirksville: Truman State University Press. 2001. Pp. xiii, 361.

The relationship between Protestantism and suicide has been made famous by the seminal work of Emile Durkheim, who based his analysis on data from nineteenth-century Europe. Over the past twenty years, historians of early modern Europe have mined the archives of England, Germany, and Switzerland to study the development of suicide; the monograph under review represents an important contribution to this scholarship.

Widely read in the social science theories of suicide—sociological, psychological, gender theory, and physiological—Jeffrey R. Watt analyzes the voluminous documentation of the former city government of Geneva. A thorough search through the relevant sources, which Watt discusses in chapter one, has yielded 404 cases of suicide between the founding of the republic in 1536 and its demise in 1798. The ensuing study is a comprehensive examination of this database that, while informed by the theoretical discussion, is distinguished by a nuanced and sensitive reading of the records. Watt first lays out the sources and methods of his study. The sometimes detailed narratives of the suicide inquests allow him not only to establish an intricate set of statistical analysis (age, occupation, class, gender, seasonality, methods of death, etc.) but also to probe the less apparent aspects of suicide (motives, cultural views, etc.). The remaining chapters focus respectively on the judicial, intellectual, social, economic, political, and the cultural dimensions of suicide.

The detailed discussion of sources and methods (there are forty tables summarizing the statistical analysis) will interest above all social historians and specialists on the history of Geneva. Watt manages, moreover, to sustain the interest of the general reader by inserting his data into a larger discussion of the political, social, and cultural changes in this preeminent Calvinist republic, so that the end result is a book that should interest historians of early modern Europe in general. Although his forte is that of a social historian, Watt also gives a competent summary of the intellectual and literary writings on suicide. He contrasts Christian disapproval of suicide with that of the ancients, and he traces the softening of the Calvinist condemnation of suicide in the Enlightenment, when reason and moderation informed not only the discourse of the philosophes but also that of the Genevan theologians.

It is impossible to summarize the many and detailed findings of this study in a short review, except to present two of Watt's most significant conclusions. First, Watt argues that Calvinism did not create a society of anxiety that led to higher suicide rates: quite the opposite. The strict and unambiguous condemnation of suicide from 1536 to the late seventeenth century, the fear of eternal damnation because of the declaration of suicide as a mortal sin, and the punitive measures of Genevan authorities (desacration of corpses, confiscation of estates, enormous fines, prison terms) against suicide together worked to restrict the

number of suicides in Geneva up to 1700. It was, rather, secularization and the decriminalization of suicide that prompted the rate to go up during the eighteenth century. Calvinist theology, and predestination in particular, Watt argues, had nothing to do with encouraging suicide. His data and conclusion are in direct contradiction to that of another study on old regime Switzerland, Markus Schär's *Seelennöte der Untertanen: Selbstmord, Melancholie und Religion im Alten Zürich 1500–1800* (1985), which uses some of the same kinds of sources and methodology. It would be intriguing to know why the two Calvinist city republics differed so much in the history of suicide, and Watt's rather summary dismissal of Schär strikes me as short-sighted and unnecessarily judgmental.

The second major conclusion in Watt's study is that a dramatic rise in suicides occurred in the second half of the eighteenth century, especially in the decade of the French and Helvetic Revolutions. A conjuncture of factors apparently led to this new regime of self-murder: the trends of secularization and decriminalization already mentioned above; an economic crisis in the Genevan economy, particularly in watchmaking; the medicalization of suicide; the removal of stigma attached to suicide; the rise of romantic love; and even the new fashion of suicide (three suicides in late eighteenth-century Geneva were inspired by the reading of J. W. von Goethe's *Sorrows of Young Werther*). If the typical suicide between 1536 and 1650 was a marginal element in Genevan society, often accused of witchcraft and demonic possession, the picture changed completely in the eighteenth century. Suicides then cut across all social classes and affected both sexes; sentiment and love played a much larger role; and suicide was well on the way to becoming the modern phenomenon that we have come to know.

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OLIVIER F. DUBUIS. *Le faux monnayage dans le Pays de Vaud (1715–1750): Crime et répression*. (Études d'histoire moderne, number 1.) Lausanne, Switzerland: Éditions du Zèbre. 1999. Pp. 216.

In recent years, the appearance of several excellent studies has greatly increased historians' knowledge of crime and criminal justice in early modern Switzerland. This book joins that distinguished company by offering readers a fascinating study of counterfeiting in the eighteenth-century Pays de Vaud in the Republic of Berne, today's Vaud canton of the Swiss Federation. Olivier F. Dubuis also offers much of interest to students of crime in other parts of Western Europe, too, because his is one of only a small number of modern studies of counterfeiting based on court records.

Dubuis selected an apt locale and period for this study. Counterfeiting flourished in the Vaud, an area that hugs the northern and western shores of Lake Geneva and that historically has been one of Europe's

crossroads. In this area of trade and human movement, the coins of many states circulated. In the early eighteenth century, amid the financial problems stemming from the War of Spanish Succession and the collapse of John Law's schemes, counterfeiting flourished, and Dubuis examines this crime from several perspectives.

Legally, the crime was a serious one, not only in the Vaud but in all of Western Europe. Counterfeiting eroded public confidence in a state's currency, threatened vital trade, and usurped the fundamental right of every sovereign state to coin money. Thus, in the Vaud, as elsewhere in early modern Europe, counterfeiting constituted a crime of *lèse majesté* meriting the harshest of penalties, including the boiling of offenders in cauldrons of oil or water. By the eighteenth century, however, Dubuis finds that Vaudois jurists, like their colleagues elsewhere, no longer applied such gruesome penalties to convicted counterfeiters. Instead, a slow-moving judicial system based in *bailliage* courts, whose verdicts were subject to review and amendment by a judicial commission in Berne, administered justice of diminishing brutality. Thus, judicial torture to gather evidence in counterfeiting cases ceased in the Vaud after 1726, just as capital punishment became an increasingly rare penalty for convicted counterfeiters. Indeed, after regulations of 1729 successfully ended the circulation of frequently counterfeited foreign specie in Berne, the criminal commission ratified only four death penalties for counterfeiting, all of them in absentia due to the flight of the criminals beyond the republic's jurisdiction. Increasingly, flogging or banishment became the state's main penalties for counterfeiting.

Dubuis also finds subjudicial responses to counterfeiting in the Vaud that suggest that the crime was far more widespread than his statistics indicate. Residents of the area displayed the same reluctance to have recourse to formal justice that historians have noted in other parts of early modern Europe. Thus, the Vaudois seem to have dealt out rough unofficial justice, abusing counterfeiters they apprehended and sometimes even extracting from the criminals legal tender that equaled the face value of the false coins the lawbreakers were attempting to pass.

Dubuis also examines counterfeiters and their practices in the Vaud. He finds that counterfeiting was overwhelmingly a crime of men in their twenties and thirties, and that their social origins were predominantly from the ranks of manual laborers, suggesting that financial need motivated some to take up this crime. Requiring both manufacture of coinage and its widest possible distribution, counterfeiting was a collaborative crime, drawing together family members or acquaintances from cabarets and other zones of male sociability. Counterfeiters produced their coins either by casting or hand striking that yielded crude output readily distinguishable from legal specie, even when counterfeiters made special efforts to reproduce the color and sheen of state coins. The public, Dubuis

shows, was aware of the pervasiveness of counterfeiting and was justifiably suspicious of all specie. The increasing accuracy of the very large coin presses employed by the state or its suppliers of coinage also made counterfeiting a risky offense; by the eighteenth century, many coins even had special markings on their edges to frustrate the practice of clipping coins.

This sparsely written, relatively brief book contributes a great deal to historians' understanding of early modern counterfeiting and the problems that it represented for the early modern state. Counterfeiting was a widespread practice in early modern Europe, as it was no more complex than pouring molten metal into a mold made by impressing a legal coin into sand. Yet it threatened the economic vitality of all zones in which it persisted, and it challenged the early modern state to develop institutional and judicial infrastructures to control it. Thus Dubuis's book will be of interest not only to early modern historians of crime and criminal justice processes but also to a broader audience.

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CHRISTOPHER S. CELENZA. *Renaissance Humanism and the Papal Curia: Lapo da Castiglionchio the Younger's De curiae commodis*. (Papers and Monographs of the American Academy in Rome, number 31.) Ann Arbor: University of Michigan Press. 1999. Pp. xiv, 244.

This book by Christopher S. Celenza provides a critical Latin edition, English translation on facing pages, and extensive scholarly introduction to the dialogue, *On the Benefits of the Curia* (1438) by the Florentine-born humanist, Lapo da Castiglionchio the Younger (1406–1438). The publication of the Latin text is notable in itself, for the only previous complete edition has been that of Richard Scholz, in *Quellen und Forschungen aus italienischen Archiven und Bibliotheken* 16 (1914): 108–53. Derived from a single source, a fifteenth-century manuscript in the Vatican Library, the Scholz version contains significant errors and misreadings. Celenza's vastly superior version is instead based on the autograph manuscript possessed by the Biblioteca Nazionale in Florence. Besides representing the source closest to the authorial final intention, this text contains numerous corrections in the humanist's own hand, enabling us to identify, as Celenza astutely comments, Lapo's efforts to clarify and, in some cases, reconsider his original wording and thoughts.

Lapo's dialogue has not been wholly ignored in modern scholarship on the Renaissance. The general tendency, beginning with Scholz, and elaborated by others, has been to view this work as articulating a secularizing humanist defense of wealth in reaction to the medieval Christian emphasis on the poverty ideal. Celenza draws different conclusions. Similar to the recent scholarly reassessments of Poggio Bracciolini's dialogue *De avaritia* (*On Avarice*), which instead of regarding it as a straightforward humanist apology for burgher values have seen it as a more problematic

moral exploration of greed and desire embedded in a largely conventional anticlerical diatribe against the hypocrisy of the priesthood, so Celenza understands Lapo's dialogue not so much as an unambiguous defense of curial ostentation but also as entailing protest against the Curia's tendencies to succumb to the corrosive effects of greed and licentiousness. Although the seeming purpose of Lapo's dialogue is an overt celebration of the Curia and a defense against those who accuse it of corruption, many of the opportunities and pleasures Lapo insists are most manifest at the Curia, including the intellectual friendships of its humanist community, invite moral compromise if not outright indulgence. The Curia's many advantages seem, in fact, to promote excesses. Thus heightened sexual desire, which Lapo apparently appreciates as the natural consequence of the many gustatory sensations aroused during the festivities of the Roman court, nonetheless elicits, he acknowledges, the notorious lasciviousness of the Roman courtesans.

These ambiguities appear even in the lengthy final section of the dialogue in which Lapo apparently defends the necessity of wealth for the curialists. The original poverty of Christ and the apostles that helped win initial converts to the new faith need no longer prevail, he argues, in the changed historical context in which the religion of Christ has become natural and inborn in the human spirit. In these circumstances, no one would respect a pauper as a member of the high clergy. The church is an institution founded by divine command and guided by the wisdom and holiness of its leaders. Integral to its majesty and power, he insists, is the wealth that is the fruit of honest human desires and that sustains the awesome splendor of its liturgical solemnities. Yet the concluding image of the Curia in Lapo's dialogue is that of the superciliousness and arrogance of curial prelates made haughty and overbearing by their positions of influence, the visage encountered by those desirous of seeking patronage there, as a frustrated and disappointed Lapo had himself done. The dialogue thus appears, in Celenza's analysis, deeply ambivalent. Lapo adopts the pose of a curial insider, which he aspired to be, frankly aware of the careerist opportunities accessible there. But his dialogue was in fact written by a humanist insistent on his intellectual and scholarly merit yet repeatedly rebuffed in his efforts to secure the privilege of placement at this leading Italian center of humanist patronage.

In its alternating impulses to celebration and vituperation, Lapo's dialogue partakes of what would become a familiar rhetorical topos in Renaissance Roman culture: the theme of praise and blame. Similarly, we encounter in Lapo's preface to his dialogue an early expression of the Roman humanist emphasis on *Roma aeterna*, of papal Rome as the eternal "monarchy of Christ." Both as an early touchstone to emerging Roman humanist cultural themes and as an expression of the problematic relations between humanists and their patrons, Lapo's dialogue is a revealing text.

Its ready accessibility in so splendid an edition marks a valuable contribution to Renaissance scholarship.

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ANN CRABB. *The Strozzi of Florence: Widowhood and Family Solidarity in the Renaissance*. (Studies in Medieval and Early Modern Civilization.) Ann Arbor: University of Michigan Press. 2000. Pp. viii, 328. \$54.50.

By the fifteenth century, widespread vernacular literacy, especially among urban dwellers, resulted in a proliferation of letters, diaries, and other personal accounts. These present a rich body of sources for private life that complement the well-documented public side of life in Renaissance cities. Yet, many of these personal sources still tell but half the story, written as they were by men. The letters of Francesco Datini, the so-called "Merchant of Prato," for example, provide a glimpse of the inner workings of a merchant's household. Yet Datini's letters present the world view of a successful merchant; we know little of the thoughts or attitudes of the women who peopled Datini's world. His wife, daughter, and the other women of his household are present but frustratingly silent supporting actors in Datini's life.

Ann Crabb's study, based on the letters of the Strozzi family, does much to right the gender imbalance of the sources. The Strozzi men had the misfortune to have run afoul of the Medici and, during their subsequent exile, there was an extensive correspondence, especially between the three sons and their mother, Alessandra Macigni Strozzi. Alessandra was the core of the family after her husband's death. She guided her sons, advised them on business etiquette, provided them with the benefit of her considerable political insights, and lobbied tirelessly to facilitate their return to Florence. She negotiated the marriages of her daughters to benefit the family and extend its network of friends and supporters. Perhaps most significantly, Alessandra provided the financial wherewithal—her dowry—that launched her sons' successful merchant ventures. All this activity is discussed in the Strozzi correspondence, which includes seventy-three letters written in Alessandra's own hand. Moreover, the extent of her activities and her effectiveness is substantiated in the correspondence of those around her, including her sons, Filippo and Lorenzo, and her son-in-law, Marco Parenti.

Crabb has used the letters, and supporting documents such as wills, tax returns, and account books, to present a thoughtful and engaging study. She sets the Strozzi firmly within the context of Florentine society to reach conclusions not only about individuals but also about prevailing social structures and values as they influenced and were reflected in the lives of individuals. Thus, she concludes that Alessandra Strozzi, while a remarkable individual, nevertheless

also reflects the normal activities and opportunities open to Florentine widows. While patriarchal society and the patriarchal family severely limited women's formal access to and influence on urban institutions, there were plenty of opportunities for women to exercise their abilities within the confines of the family. Moreover, widows like Alessandra were able to navigate the public sphere without risking their honor, as well as to utilize personal and private connections to influence events. The letters reveal Alessandra negotiating with government officials to reduce the heavy tax burden on her family's estate and using her personal and social networks to influence the Medici to repeal her sons' exile. Crabb argues these were not atypical of Florentine women's opportunities, activities, and strategies.

The Strozzi correspondence also reveals the concept of family solidarity and how people viewed themselves as members of a *casa*. The predominance of patrilineal affiliation is reinforced, but Alessandra also maintained enduring ties with her Macigni relations. She and other married women did not hesitate to call upon the assistance or influence of their natal families. While the letters provide evidence of Alessandra's love for her husband and sons, her relationship with her daughters is more opaque. Crabb suggests that this may reflect the fact that her daughters first lived with her and then, after marriage, remained in Florence. Their interaction was unbroken by the separation and anxiety of exile. Nevertheless, Alessandra embraced the ideal of the patrilineal family. Her goal was to end her sons' exile so that she could spend her old age living with her eldest son, his wife, and their children in the patrilineal, multigenerational household that was the patrician ideal.

Significantly, too, Crabb argues that while Filippo and Lorenzo viewed themselves as members of the Strozzi *casa*, they also saw themselves as independent individuals. Thus, while Filippo employed Strozzi relatives and maintained his businesses with his brother, cousins, and more distant kin, he viewed his accomplishments as personal achievements. In maturity, as arguably the most successful Strozzi and the richest man in Florence, he saw himself as a member of a patrilineage but also as the central and leading member of his generation. Crabb terms his perspective as "familial individualism" (p. 233), which nicely reconciles the individual autonomy and family solidarity that is revealed in the letters.

This is a congenial study of an individual family and of a society. The personalities and experiences of the Strozzi and their relations are at the heart of the book. There is a strong narrative that recounts the story of the Strozzi, their exile and return to Florence, and the amassing of their fortune. This is overlaid with an account of the emotional relationships among members of the family. Finally, the Strozzi are presented in the context of their age, as embodying the ideals of honor and profit, the patrilineal extended family, the merchant spirit, and the merchant experience. While

neither a microhistory nor a social history, Crabb's careful treatment of the Strozzi is an excellent introduction to Florentine society and a Renaissance family.

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MONICA CHOJNACKA. *Working Women of Early Modern Venice*. (Johns Hopkins University Studies in Historical and Political Science.) Baltimore: Johns Hopkins University Press. 2001. Pp. xxii, 188. \$32.50.

If social history is the study of those below the elite, history "from the bottom up," then Monica Chojnacka has brought a truly social historical approach to the study of Venetian women in the sixteenth and seventeenth centuries. By carefully exploiting records of the Venetian Inquisition, the *status animarum* (parish registers), and more typical notarial and fiscal documents, she has succeeded in tracking women's activities in a range of arenas. Deliberately eschewing "the traditional structure of examining women's history along the stages of their life cycles" (p. xvii), she has instead used a geographic and institutional framework for her analysis. Venetian *popolane* are viewed against the backdrop of household, neighborhood, and city. The resulting image may be static chronologically (in terms of Venetian history and in terms of women's life cycles), but it is spatially dynamic.

Chojnacka's approach enables her to argue that Venice "belonged to its women as well as its men" (p. xvi) and that these women exercised genuine social power as independent agents. Here her findings are unexceptional, in line with much of recent women's history. What makes her work stand out is the tracing of women's power beyond the household and family, although she begins there. Her analysis of households is mainly statistical and shows that "there was no one single living pattern for Venetian women and men" (p. 25). Marital status (as nubile, married, or widowed) of course affected housing options, including living arrangements involving one's kin. Married couples tended to establish their own homes and to retain fewer ties to their natal families than is usually supposed for southern Europe. Chojnacka tends to equate the situation of single and widowed women, whereas life cycle-based studies (the work of Giulia Calvi on Florentine widows, for example) have pointed repeatedly to greater independence and responsibility on the part of widows. Chojnacka is not unaware of this, but she is also not out to emphasize it.

Against the well-known financial power of women exercised through their wills (as studied by Stanley Chojnacki), Chojnacka in chapter two plumbs the records for evidence of control and direction in more mundane and frequent uses of property. Leases and other records of management, tax records showing women holding property as their own, judicial debt records: all show that "women could own and manage goods and property themselves, independently of their

fathers, husbands, brothers, or sons" (p. 41). Women in all stages of life held and managed property, although such women were always a clear minority.

Such women were also prominent in their neighborhoods. Some Venetian *contrade* were heavily populated by female heads of household, including prostitutes. Testimonies before the Inquisition reveal a dense web of female activities on the streets, which Chojnacka compares to the less active presence of men. Neighborhood women turned to each other for help and advice as regularly as they exchanged gossip. Most fascinating in this third chapter is description of the "porous boundaries" to spaces, such that neighbors seem routinely to have entered each other's homes or looked within, even through holes in the walls. Of course, the sources here lend themselves to narrative rather than statistical treatment. Chapter four demonstrates the important presence of immigrants in Venice: women who found the city, it seems, a fairly hostile place. They were frequent targets of neighbors' animosities (in the form of denunciations to the Inquisitors).

Home and neighborhood have often been taken as female spaces, with the wider city seen as the domain of more mobile males. Chojnacka shows that women too daily traversed the city for all sorts of business (including to testify in courts); and they moved to other neighborhoods, not just when married but for work. Women also journeyed beyond Venice to visit kin and friends or even as exiles. In comparison to more home-bound patrician women, who dominate the usual sources, humbler women had to range more widely to get by.

In the sixth and final chapter, Chojnacka investigates the charitable institutions founded in Venice in the second half of the sixteenth century. Outfits such as the Casa delle Zitelle (for young girls) and Casa del Soccorso (for repentant prostitutes) provided services to women and required services from women. They utilized the enclosure typical of convents for those in need and the patronage of wealthy benefactors, including women, as was typical in the neighborhoods. Chojnacka sees these places not as Foucauldian prisons but as means to restore and enable women to return to social life beyond the enclosure in a "citywide network of charity, adoption, marriage, and employment" (p. 137).

This is an important, pioneering study. It supplements the work on Venetian patrician women, although it perhaps leaves these latter too far out of view to allow readers to draw clear contrasts between the lives of the privileged and those of the humbler women. The book gives us a Venice rather different from that found in so many other works of Venetian history, less exceptional in comparison to other communities; but it also does not pursue many overt comparisons to the fine work done by historians on women in Rome, Florence, Bologna, and elsewhere.

That may well be the next step. Chojnacka can turn her fine abilities, as can others inspired by her, to a broader and more comparative framework.

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RUTH BEN GHAT. *Fascist Modernities: Italy, 1922–1945*. (Studies on the History of Society and culture, number 42.) Berkeley and Los Angeles: University of California Press. 2001. Pp. x, 317. \$45.00.

This book takes on the very difficult task of disentangling the close connection Italian fascism established with the cultural realm during Benito Mussolini's regime. More specifically, and in an original way, it explicates the mutual relation of influence that underlay the making in Italy of both fascism and culture. That process, Ruth Ben Ghat rightly claims, was not unidirectional, and we can gauge its course by analyzing the parallel history of the regime's and the intellectuals' identity construction during the twenty years of Mussolini's rule.

The book's main claim is that the intellectuals actively participated in fascism's fantasy of national regeneration and international success, and went along with, and supported, the key fascist concept of *bonifica* (reclamation) as a metaphor for Mussolini's totalizing plan to transform Italy and the Italians. Weary of what they considered extreme models of modernity, Italian intellectuals saw in fascism a modern promise of change. The fascists, as the regime liked to sustain, would be able to install modernity without its degenerative consequences of mass standardization by maintaining the rules of tradition and national differences. Modernity did not need to be rejected. Indeed, in the eyes of many intellectuals, the valence of modern sites such as cinema, colonies, and also some artistic styles, once examined within the fascist context, changed from negative to positive. Realism would be a case in point.

In six dense chapters and an epilogue, Ben Ghat cogently revisits the established notion of Italian intellectuals' presumed innocence under fascism and problematizes the different dimensions of their fascist belonging. She questions the identification of progressive cultural views with a diminution in fascist allegiances, the separation between "bad guys" from "good guys" among fascist officials, for example Giuseppe Bottai. Her account also brings a new twist to the history of fascist culture based on the notion of "pluralism" by arguing that fascism's seemingly liberal attitude toward the arts actually managed to enlist even more intellectuals in the regime's cultural initiatives and opportunities. Because the regime never adopted an official aesthetic stance, intellectuals could convince themselves they were not producing political art when they sponsored the idea that popular contact with aesthetics would create new modern Italians. Thus, in the shade of their Crocean ideal, they pro-

duced art in the regime and for the regime, supporting fascism's several projects over the years. Tolerance (which also promoted self-censorship) paid off, Ben Ghiat claims, and the regime was well aware of that. The regime was not just being inefficiently open and generous, "pluralistic" (hence the inappropriateness of the term): it was being strategic.

By analyzing literature, films, and youth journals especially, Ben Ghiat offers more detailed accounts of the subtle ways in which cultural practitioners negotiated their identity within the regime's demands, and of how the regime, which needed the intellectuals' support to pursue its goals, in turn responded to their works. Her model gives birth to a panorama of Italian intellectuals under fascism displaying in varying degrees a combination of opportunism and authentic belief in the regime. The book, however (and this is the main criticism I would raise), does not systematically provide a grid through which to categorize intellectuals under fascism. Who were the true fascists, or the true opportunists, or were all intellectuals both opportunist and fascist? What defines one as a fascist: practical political action, ideologies, or cultural assumptions? These are complex questions and hard to resolve. (Interestingly, the author does have a footnote in which she embraces Alexander von Plato's three-tiered categorization of young intellectuals; see chapter six, note 30.)

In the end, Ben Ghiat's main argument remains that the intellectuals under fascism were generally guilty of complicity with the regime. They shared its concern with an ethical literature and ethical cinema, and took the knowledge of other cultures—the regime's openness to internationalism—as a way to reaffirm the spiritual superiority of the Italian fascist model. They adopted a realist aesthetics that privileged the local aspects of modern existence and rejected foreign models of modernity. When the Ethiopian War broke out and the racial element, along with autarchy and anti-Semitism, became overriding concerns in the regime, the intellectuals embraced those causes, later becoming disillusioned but never renouncing fascism and Mussolini. Ben Ghiat recognizes that fascism's tolerance for the arts did open up spaces for critiques that later gave way to antifascist activities. But against the contemporary trend, at least in Italy, that explores the presence of a fascist "left," she prefers to bring attention to the Italian intellectuals' amnesia about their political responsibilities and their collaboration and compromises with Mussolini's regime. Italy's failure to collectively reckon with its fascist past, she concludes, casts a shadow over post-1945 Italian culture.

Thought-provoking and challenging, this book courageously tackles intractable issues in the history of Italian fascism. It adds new insights to our understanding of the relationship between fascism and culture and constitutes a noteworthy addition to the literature

on fascist modernity and to the larger debate on politics and the arts.

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ISTVÁN GYÖRGY TÓTH. *Literacy and Written Culture in Early Modern Central Europe*. New York: Central European University Press. 2000. Pp. 266. \$41.95.

"Your Lordship has asked me if I knew how to write in Hungarian," wrote Borbála Kerhen in 1556; she answered that she, growing up, "did not hear that girls or women learned how to write in this way," noting further that "my poor mother indeed would have had me tutored had others been tutored, too, but we did not even hear about it" (p. 123). István György Tóth focuses on precisely such historical thresholds as this one, when the inquiry about literacy might have seemed plausible to the questioner, but the spread of literacy was still so uneven that the answer might often be modestly negative. In fact, this cultural frontier existed across the early modern centuries, as advances in literacy occurred at different rates: landed nobles before lesser nobles, men before women, nobles before peasants, while varying also from region to region. Tóth's impressive scholarly achievement is to apply a measure of science to these crucial historical issues, producing persuasive statistics on schooling for at least one county in western Hungary, Vas County, in the eighteenth century, and then attempting to interpret those statistics in the context of information from other counties of Hungary and other regions of Europe. Much more than just measuring the advance of literacy, however, Tóth ambitiously attempts to corollate the shifting balance between oral and written culture in Hungary and to study the social ramifications of such development. First published in Hungarian in 1996, this book is a model study of scrupulous rigor in its numerically minded social analysis, of challenging methodological innovation in its approach to the problem, and of marvelous insight into the social and cultural meaning of early modern literacy and illiteracy.

The first chapter focuses on Vas County and attempts to calculate the percentage of children who attended school in the 1770s, the decade of Maria Theresa's school reform, *Ratio Educationis* (1777), which mandated some measure of compulsory elementary education. Tóth estimates that between fifteen and twenty percent of children received some schooling in the 1770s. He further surveys some of the limiting factors on the effectiveness of reform, most notably the irregular qualifications of most teachers, especially in Latin and arithmetic. The second and third chapters consider the cases of the peasantry and the nobility, respectively. In the case of the peasantry, widespread illiteracy made reading seem a magical and sometimes diabolical skill. Eighteenth-century witchcraft trials involved testimony about the devil writing

in his book the names of illiterate women whom he enrolled as witches. Tóth argues that in the eighteenth century a cultural disjunction emerged between the new importance of written documents and the persistent inability of illiterate peasants to read the documents that shaped their lives. Military discharge papers, prepared by the authorities, might be illegible to the bearer: "I would not know what they had written and what not; I myself know not writing and reading, and they wrote as they liked to" (p. 85). Counting signatures and crosses from documents in estate archives, Tóth estimates that in Vas County between two and three percent of male peasants were literate in the 1780s. The Hungarian nobility confronted the emerging modern world of written culture with better percentages but not with general literacy; Tóth's statistics for the lower nobility in Vas County record a rise in literacy from twenty to almost forty percent over the course of the eighteenth century.

In the fourth chapter, Tóth focuses on the "oral tradition" in early modern Hungary and achieves brilliant results through the methodologically inspirational approach of examining ancestry cases. In these judicial cases, Hungarians attempted to prove their family's noble status with reference to patents of nobility. The patents were written documents (written, grandly, in gold) but sometimes centuries old, so that the supposed nobles had to try to prove through oral tradition and living memory that they did indeed belong to the same family to which the patent originally applied. Such cases, Tóth suggests, were characteristic of Hungarian society in the eighteenth century, after the great Ottoman wars of the late seventeenth century; in peace, under Habsburg rule, claims to noble privilege were judicially evaluated, especially since the turmoil of war had often separated patents from their rightful owners, putting the precious papers on the black market. For each disputed patent, it was necessary to try to establish who had seen it in the hands of its rightful owners, and, of those who had seen it, who had been able actually to read it. In this way, the ancestry cases offer valuable evidence concerning literacy, as well as nobility, in Hungary, and also demonstrate the complex interplay between the written documentary culture represented by the patent and the oral culture invoked in the memories of witnesses. It was even possible for oral remembrance, in court, to attempt to establish the connection, across generations and centuries, between a family in 1740, the year of the accession of Maria Theresa, and a patent issued in 1571 by Maximilian II. A ninety-three-year-old serf claimed to remember geriatric reports from his own childhood that spanned the entire period.

Tóth's work provides an important complement to James Van Horn Melton's research on education in the eighteenth-century Habsburg monarchy and also contributes to the theoretical appreciation of the history of reading and print culture, which has been notably advanced by Roger Chartier and Robert Darn-ton. In a fifth and final chapter, Tóth extends the

discussion into the nineteenth century and sketches some of the regional variations in literacy within Hungary and across Europe. In conclusion, he notes the political implications of literacy on the threshold of modern history, when the Habsburg Palatine of Hungary in 1795 worried that literate peasants would "waste time reading papers, books, and pamphlets"; around the same time, an eighteenth-century Neapolitan traveler in Hungary was impressed by local literacy and declared that even chambermaids, "books in hand, argue about the laws of nature and all that concerns the rights of mankind" (pp. 209–211).

Tóth, in this study of literacy, has made an exceptionally important contribution toward understanding culture and society in early modern Europe, focusing on a crucial aspect of modernity and analyzing its development and ramifications: from the time when Borbála Kerhen's poor mother would never have dreamed of teaching her daughter to write to a time when a chambermaid, book in hand, might have learned to articulate the rights of men and women.

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KATALIN PÉTER, editor. *Beloved Children: History of Aristocratic Childhood in Hungary in the Early Modern Age*. Budapest and New York: Central European University Press. 2001. Pp. 271. \$39.95.

In the 1970s and 1980s, there was keen and sometimes acrimonious debate about the quality of the relationships between early modern parents and their children, but for fifteen years or more now there has been a broad consensus that most parents loved their children according to their lights, worried about their illnesses, mourned for them if they died, chastised them within understood limits, and tried to provide them with means of support both within the parents' lifetimes and afterward, through their wills. The title of this book suggests strongly that it fits within this parameter, and editor Katalin Péter's own contribution, particularly in a long chapter on "The First Ten Years of Life," provides convincing evidence in support. Relying primarily on day-to-day details in the correspondence between family members, Péter introduces the reader to a culture of child rearing, involving fathers as well as mothers, which was caring and loving, and in which differences between Protestant and Catholic, so often pointed up in the literature on Western Europe, were minimal.

But this is a book about the aristocracy, and the existing historiography has often noted that in some respects aristocrats failed to conform fully to the caring parent stereotype. They were, for example, particularly concerned about the preservation or enlargement of their estates, and about the production of male heirs; intervals between births were on that account short. In the remaining three chapters of the book (on orphans, a case study of Count Ádám Batthyány I's treatment of his sons, and the marriage

policy of the Esterházy family), it is aristocratic imperatives that loom largest. The fate of orphans depended in essence on how valuable they were in terms of possessions or name to those more distant family members charged with their care. "Guardians," we read, "often spent the orphans' ready cash and sold their movable property" (p. 146). Batthyány certainly took pains over his sons' education, sending them off with a large retinue to schools, and monitoring their progress, or rather the lack of it, but that hardly sustains the assertion that "he loved his family dearly" (p. 163)—though the chapter is certainly an interesting study of changing norms in the rearing of well-born children in the course of the seventeenth century. As to marriage, girls were routinely married off at the age of fourteen (peasant girls by contrast married in their late teens or early twenties), their own wishes never consulted, and children were mass produced (the twenty-seven children born to the two wives of Pal Esterházy probably constituting a record).

The different tone of the two halves of the book, and the different conclusions that are likely to be drawn, reflect in part different types of source and different types of question being asked of them, but they are a rather jolting reminder that the now discredited view that childhood in the early modern period was characterized by adult cruelty and lack of care had something in its favor. Certainly no one confining his or her reading to the last three chapters would have dreamed up the title given to the book as a whole: on this evidence, these were not "beloved children." The book, then, provokes as many questions as it answers, and it is a pity that the editor did not confront some of them. In her introduction, she backs away from consideration of how a study of the child-rearing practices of the Hungarian aristocracy relates to the existing studies of aristocracies elsewhere. Aristocrats have been chosen for study simply because they have left more, and more manageable, records, than other social groups, rather than because they might have had distinctive child-rearing practices: the assumption seems to be that a study of them may tell us something about Hungarian society as a whole. The research embodied in this book, based on extensive and revealing records, sheds some fascinating shafts of light on the Hungarian aristocracy in the sixteenth and seventeenth centuries, but the picture presented is not internally consistent, nor is it well integrated into the wider European historiography of parent-child relations or family forms.

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ALICE FREIFELD. *Nationalism and the Crowd in Liberal Hungary, 1848–1914*. Baltimore: Johns Hopkins University Press and Woodrow Wilson Center, Washington, D.C. 2000. Pp. xii, 398. \$45.00.

Michael Polanyi, the distinguished physical chemist turned philosopher, always looked back with thankfulness to the liberal Hungary into which he was born in 1891. He was not unduly disturbed by the fact that Hungarian liberalism, as it had evolved after the Austrians and Russians suppressed the Revolution of 1848–1849, was a liberalism of the Right, not of the Left; it never pretended, for example, to harbor any sympathy for democracy. Largely for that reason, historians have generally adopted a critical attitude toward it. But after the experience of communism, Hungarians have begun to view the liberal era, whatever its limitations might have been, in a new light. In this subtle and highly original study, Alice Freifeld takes sides with the contemporary Hungarian historian Ferenc Glatz, who has argued that "1867–1918 was the high point in modern Hungarian history, the period in which Hungary came closest to the Western European standard of culture and development" (p. 23).

Freifeld brings liberal Hungary to life by focusing her attention on "the chastened crowd" that replaced the revolutionary crowd of 1848–1849. The former, she suggests, holds the key to a fuller understanding of how Hungarian nationalism managed to achieve many, if not all, of its aims by repudiating radicalism and adopting a more cautious, and effective, liberal strategy. To be sure, many liberals—like all conservatives—regarded crowds with suspicion, but some in Hungary recognized their nation-building, even their civilizing, potential. And they did so without discounting the crowd's ability to force the pace of reform.

During the Alexander Bach era, Freifeld shows, the national-liberal crowd played a waiting game, finding a real opportunity only in 1855, when the nation gathered to mourn the loss of its greatest Romantic poet, Mihály Vörösmarty. Although nervous neo-absolutist authorities were certainly on the lookout for trouble, they could not help but be impressed by a crowd that conducted itself in a dignified and peaceful manner. Two years later, Emperor Franz Josef made a tour of the country that allowed him to establish a fragile but real bond of affection with the crowds that welcomed him and with the nation at large. Anonymous Hungarians, Freifeld observes, were beginning to view 1848–1849 as a war of independence, not a revolution. In other words, they, together with such luminaries as Gyula Andrássy, Ferenc Deák, and József Eötvös, were beginning to prepare the way for the Compromise of 1867.

Freifeld devotes a fine chapter to the celebrations, especially the emperor's coronation as Hungarian king, that served to legitimize the Ausgleich. 1848 was not to be forgotten, but memory of it was to be transmuted and integrated into a new, more evolutionary, national consciousness. Perhaps no one reflected that consciousness more completely than Kálmán Tisza, the right-wing liberal who reconciled the camps of 1848 and 1867 and guided Hungary's destinies from 1875 to 1890. Like other liberals, Tisza recognized the

uses of crowds, not only at funerals of national heroes but at carefully planned public exhibitions as well.

Freifeld does not ignore the less attractive side of Tisza's Hungary; she calls attention, for example, to the Magyarization campaign that has drawn so much criticism. But she also reminds us that it eschewed racialism and absorbed virtues associated with the principal groups being assimilated: the Germans and the Jews. Indeed, at a time when anti-Semitism was on the rise in Austria, the Tisza government was philo-Semitic and uncompromising in its opposition to the anti-Semitic political movement that sought to capitalize on the base passions excited by the "ritual murder" trial at Tiszaeszlár in 1882.

Anti-Semitism was not, of course, the only threat to Hungarian liberalism. By the time Tisza died in 1902, socialism had all but taken control of the streets. When war broke out in 1914, the chastened, liberal crowd staged its last hurrah. Or was it the last? In an epilogue, Freifeld points out that belief in the enduring value of the chastened crowd, especially as an antidote to totalitarianism, never died. It manifested itself in 1956 at the reburial ceremony for László Rajk, a victim of the Hungarian Communist purge. And it accounts for the day in June 1989, when a peaceful but resolute crowd of a quarter-million people turned out to rehabilitate and rebury Imre Nagy, the martyred leader of the 1956 revolution, and thereby accelerated the "negotiated revolution," the nonviolent transition to the postcommunist era.

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JAMES GOW and CATHIE CARMICHAEL. *Slovenia and the Slovenes: A Small State and the New Europe*. Bloomington: Indiana University Press. 2000. pp. xi, 234. \$39.95.

This is a book in which James Gow and Cathie Carmichael, two Slavic specialists, introduce, in all its complexity, a small state in the new Europe: its geographical variety and natural beauty; the origin of its people; their history, which, ten years ago, fought about the creation of the Slovene independent state; and postindependence developments in the main areas of social life. The book seems to have two parts. In the first, shorter part (two chapters), the authors briefly describe the main developments that led to the formation of the Slovenian nation within Austria and later the Austro-Hungarian Empire and then focus on the state-forming processes of the Slovenes in Royal (1918–1941) and Communist Yugoslavia (1945–1990). In the second, more extensive part, four thematic essays on culture, economy, politics, and security are presented. While the chapter on culture deals entirely with its historical aspects before 1990, the other three have a more contemporary focus, discussing mostly, although not exclusively, the 1980s and particularly the 1990s. Much information in the book, especially about the last two decades, has been published in various

English publications dealing with Slovenia or postcommunist societies in the 1990s; however, the book offers new data, and certainly the authors' own view of the country's development.

The book is interesting and well written. Both authors learned the Slovenian language and spent some time in the 1980s and 1990s studying in Slovenia, which enabled them to gain firsthand knowledge of the country and also experience its distinctive characteristics. Gow and Carmichael are well aware of the importance of the Slovenian language and culture for the survival of the Slovenes as a distinct ethnic group and their significant role in the formation of the Slovenian identity as a nation and a state. In their analysis, the authors identify "the crux of the Slovenian cultural dilemma: further development, as well as recognition in global eyes, depend on embracing internationalization while finding a way to maintain tradition" (p. 61). This dilemma is present also in other spheres of Slovenian life, such as economy and security.

The authors see Slovenian cultural distinctiveness as a strength and a challenge but above all as a hindrance to the processes of integration into the European Union and the North American Treaty Organization (NATO), a position certainly not unique to the Slovenes. The nation's culture as a hindrance to the integration processes is, at least to my Slovenian eyes, overemphasized: economic and security integration do not require loss of the national identity nor do they necessarily lead to its loss. I am not quite sure what the future of the European Union and NATO will be. In the interest of human survival and quality of life, we want to preserve the specific characteristics not only of individuals but also of larger groups and, at the same time, develop understanding and respect for differences among peoples and cultures. Therefore I do not see the integration of Slovenes, or any other small nation into the European or world community, in terms of one-way adaptation, of giving up distinctiveness considered a nuisance in profit-making schemes. A more harmonious life among nations and states in Europe should be striven for rather than purely economic and geopolitical goals.

Although the reader may at times find the selection and the interpretation of events or personalities dealt with in the book unusual, especially in the chapter on culture, Gow and Carmichael deserve praise for their extensive research and interdisciplinary approach to Slovenia and the Slovenes over time. The book is a solid narrative "view from outside," which is useful not only for English-speaking readers learning about Slovenia but also for Slovenes as they reflect on their state and themselves.

Having said that, I cannot overlook some minor but at times irritating problems. It is upsetting to find so many misspelled Slovenian words, proper names in particular, both in the text and in the index; a few incorrect translations; and incorrectly identified publishers' locations. Nor do I see much reason for the

authors' decision (p. x) to use English capitalization standards for citations of Slovenian sources. Why not retain the actual form of the Slovenian originals? Such flaws can easily be corrected in the next edition to give the book the appearance of being the scholarly work that it is.

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CHESTER S. L. DUNNING. *Russia's First Civil War: The Time of Troubles and the Founding of the Romanov Dynasty*. University Park: Pennsylvania State University Press. 2001. Pp. xiii, 657. \$65.00.

Chester S. L. Dunning's new book is a historical tour de force, a major reconceptualization of Russia's Time of Troubles, 1598–1613. In 1598, Dunning argues, Russians were confronted with the death of a royal dynasty. At the same time, the ordinary cavalryman of the Russian army—who was awarded estates (and peasants) in return for loyal military service—was pushed to extremes by agricultural disaster and the flight of peasants from his and the state's growing demands. Finally, Cossacks, on or near the fringes of the Russian state, absorbed fugitives from the center's economic and political disaster, developing into a powerful adjunct military force. An immediate event, the arrival of a pretender on the heels of devastating famine, sparked more than a decade of full-scale civil war, comparable in importance and implication to those of early modern England or France.

This nuanced and colorful account is broadly cultural-political in its focus; it argues directly against the long-established and influential Soviet interpretation that the Time of Troubles was predominantly a war between social classes over the development of serfdom. Elements of the Soviet interpretation have certainly been challenged implicitly and explicitly over the past fifteen years. This book goes much further. It relies heavily on recent research and on a detailed examination of sources to construct a thorough-going reevaluation of the period: civil war not class war.

The key to Dunning's reinterpretation necessarily lies with Tsar Dmitrii Ivanovich, who appeared on the Russian border in 1604 claiming to be the miraculously surviving son of Ivan IV by Maria Nagaia (it seems most unlikely that his claim was true). Remarkably, there was dedicated popular and military support for him from his appearance until his assassination in 1606 at the behest of his successor, Vasili Shuiskii. Many of Dmitrii's followers then fought on determinedly after 1606, under the leadership of men like Ivan Bolotnikov, for a second, imposter Dmitrii. Dunning argues Russians' dedication to the first Dmitrii stemmed from their political beliefs in sacral kingship, not from Dmitrii's championship of a social cause. The qualities of sacral kingship could easily be attached to the behavior, the life, and the lineage of Dmitrii, but not to Boris Godunov or Shuiskii, for example. Dmitrii was

neither a conscious imposter nor a Polish patsy but embodied the true tsar; his defenders' devotion even led them to hope that a second Dmitrii could miraculously fulfill their ideal. Only toward the end (1610), when unruly Cossacks came to play such a large role, did Dmitrii's elite support begin to drain away. Further complications ensued from both Polish and Swedish interventions. Careful management of the Romanov accession to the throne gradually restored order; Dmitrii's claims were explicitly rejected. Social and political complications deriving from the Troubles continued to plague an expanding fiscal-military state in Russia throughout the seventeenth century. An unresolved question, however, nags at this reader. If Dmitrii was not a defrocked monk named Otrepov, who was he, who "managed" him, and how could he have been unaware that he was an imposter?

This a powerful and carefully presented argument. It has the attraction of taking seriously the mindset and opinions of its subjects. It also triumphantly unites new interpretational aspects of recent pre-Petrine research (including some that this reader had previously thought incompatible) to present greater continuity between the Troubles and the military-fiscal state that followed; many of these new interpretations are Western in origin. In this way, and by references to comparative work, Dunning consciously links Russian historiography with that of early modern Europe, where the role of social class in political upheaval has been a matter of ongoing debate. His emphasis on social diversity among Dmitrii's followers and his rehabilitation of Dmitrii are convincing. His rejection of the class warfare thesis involves an often stern reconsideration of his predecessors, the evidence they use, and explanatory concepts such as "naïve monarchism" and "pretender-ism," when used imprecisely. Most important, he argues forcefully that most of Dmitrii's followers were military men and not peasants or serfs at all. The evidence identifying Dmitrii with peasant rebellion, he argues, is based on anti-Dmitrii propaganda.

Dunning avoids the temptation to react too vehemently against a historical model both monolithic and so long unchallenged. He cites evidence of different lines of social stress, between wealth and relative poverty and within the military. Economic and social questions are not as deeply integrated into the analytical framework of the latter part of the book, which discusses the period at which their impact was the greatest. That said, Dunning successfully uses a variety of approaches, from studies of climate and demography to comparative state-building, to reevaluate the existing evidence and produce a major historical reinterpretation.

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MARSHALL T. POE. *"A People Born to Slavery": Russia in Early Modern European Ethnography, 1476–1748*. (Studies of the Harriman Institute.) Ithaca: Cornell University Press. 2000. Pp. xi, 293. \$49.95.

As Marshall T. Poe observes in the introduction to this extraordinarily well-organized, exceptionally well written, profusely illustrated, and handsomely produced monograph, historians of Muscovy have always been ambivalent about the utility of accounts written by Europeans, valuing their unique information but remaining skeptical of their biases and ignorance (p. 5). Poe presents a comprehensive analysis of their view that the tyrannical ruler of Muscovy had unlimited power, owned all land, and abased even his noble subjects.

Poe rationalizes his title by explaining that most of the sources were "ethnographies," or surveys of a region according to a template of topics, although the word is an anachronism (pp. 5–6). Nor did the travelers call themselves "Europeans," which Poe selects as a lesser evil than "Westerners," since the standard early modern view was that Muscovy was the "North," not the "East" (pp. 8–9). His flexibility in selecting his sources is illustrated by his quite justified inclusion of the Croatian Catholic priest Iurii Krizhanich (Poe uses the Russian spelling), who wrote in Siberian exile, and the distinctly non-European Paul of Aleppo, a Syrian Arab archdeacon who wrote in Arabic.

The earliest accounts, based upon very little information and often written by people who had not even visited Muscovy, contained both positive and negative images of Muscovy. The accounts of short-term envoys and itinerant merchants and long-term residents in Muscovy more often propagated the image of the Muscovite ruler as a tyrant dominating his slave-subjects. This theory did not become dominant until the 1550s and 1560s, when during the Livonian War Polish and Livonian anti-Muscovite propaganda about Muscovite atrocities, reinforced by stories of Ivan IV's *Oprichnina*, motivated the much more widespread dissemination of Sigismund von Herberstein's account, written not about Ivan IV but his predecessors, which depicted Muscovy as a tyranny. Indeed, mistranslation led to the rendering of Ivan's epithet as "the Tyrant," not "the Terrible," to such a degree that folkloric atrocity tales, previously attributed to Vlad Tsepes, aka Dracula, became attached to Ivan IV (see appendix for a survey).

Ethnographers faced an intellectual dilemma, since Aristotle had taught them that a tyrant was an exception whose unpopularity would destabilize his regime. Although Muscovite rulers after Ivan were not tyrants, their oppressive regime remained stable, an unresolved contradiction inadequately resolved by calling Muscovy a despotism, a tyranny without a tyrant. Poe discusses how political scientists from Jean Bodin to Montesquieu categorized Muscovy by adapting the very same Aristotelian categories that shaped the perceptions of the Renaissance humanist authors from whose accounts they drew their data.

In his final chapter, Poe attempts to validate this assessment of Muscovy. Poe "translates" the accounts' exaggerated binary distinction between European liberty and Muscovite servility, which made Muscovy

typologically distinct, into the poles of a continuum of early modern European monarchies in which Muscovy was indeed located at the extreme where the monarch had more power, private property was less secure, and the population owed more service. Discarding as cultural prejudice the very notion of inborn servility, Poe nevertheless argues that the Muscovite rulers did impose upon their people a discourse of servility, which the Muscovites gradually took rhetorically and without servile connotations while the Europeans misconstrued it literally. Servility in Muscovy did carry mutual (but not contractual) obligations, for the ruler had to protect his slaves-subjects. Poe replaces Aristotle's terminology with Max Weber's "patrimonial state" and argues that the Muscovites genuinely believed that autocracy ameliorated the dangers that faced all early modern European monarchies, something the Europeans could not imagine. "Muscovite rule was not, as the foreigners thought, a faulty, deviant, or illegitimate form of government imposed by a ruthless king," Poe concludes, but a successful and creative adaptation to local conditions (p. 226).

This is a superlative monograph, with an elegantly constructed, tightly argued thesis. It clearly establishes the genealogy of the notion of Muscovy as a tyranny in the European accounts, and the decisive role therein of the least typical Muscovite ruler, Ivan IV, and integrates that development with European political theory. Poe brilliantly traces the pervasive influence of Aristotle upon visitor and theoretician alike, placing Giles Fletcher's famous prediction of civil unrest after Ivan IV's tyranny in its full intellectual context.

Poe concedes that the European accounts were often biased, erroneous, fantastic, fictitious projections of their authors' fears and expressions of their religious, class, and ethnic prejudices, but he nevertheless contends that the travelers were not naïve and accurately portrayed their experience in Muscovy. Perhaps the only contradiction Poe fails to mention is that the Europeans insisted that Russians could not be trusted to tell the truth (pp. 51, 91), yet they authenticated their accounts by invoking trustworthy testimony from native informants (pp. 52–53, 93, 122).

Poe attempts to synthesize European accounts about Muscovy and current scholarly understanding of its government. However, his emphasis on the lack of constitutional constraints on the power of the Muscovite tsar and of corporate estate rights runs the risk of falling into whig historiography. Poe discusses the moral obligations of the ruler for his servile subjects but does not discuss the role of custom (*starina*) in regulating political behavior. His conclusions should stimulate much useful debate about Muscovite society and politics.

Poe's book is certainly the definitive account of the evolution of the theory of Muscovy as a tyranny in early modern European ethnography.

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CHRISTINE D. WOROBEC. *Possessed: Women, Witches, and Demons in Imperial Russia*. DeKalb: Northern Illinois University Press. 2001. Pp. xv, 288. \$36.00.

Christine D. Worobec investigates what appears to be an obscure phenomenon of the Russian past: *klikushestvo*, or demon possession. Recorded as early as the eleventh century but occurring with increasing frequency after the sixteenth, *klikushestvo* involved individuals—most often peasant women—who would writhe, scream, and engage in strange public displays that onlookers usually interpreted as evidence of bewitchment. Peasants took these episodes very seriously, seeking help for the possessed from faith healers and exorcists and, to the chagrin of those charged with maintaining public order, often attacking the witch or sorcerer deemed responsible.

Worobec, however, is less interested in *klikushestvo* per se than in how Russian educated society used this phenomenon to understand the peasantry, particularly peasant women, and impose its values on what was perceived as the dark countryside. Consequently, she examines the policies toward demon possession and the various interpretations of it, starting with Peter the Great's attempts to root out fakers and moving through the views of nineteenth-century Russian Orthodox clergymen, Russian writers, ethnographers, and, finally, psychiatrists. Most of these authorities, even some clergymen by the mid-nineteenth century, came equipped with rationalist approaches to the irrational. As a result, they were ultimately unable to treat demon possession as much more than a metaphor for the backwardness of the countryside and the long suffering of the Russian peasantry. By the end of the nineteenth century and particularly following the Revolution of 1905, *klikushestvo* came to represent "a peasantry so ignorant, backward, primitive, and destructive that it had the power to destroy everything that was progressive and Western in Russian society" (pp. 146–47).

In her book, Worobec takes us through the laborious efforts of the elite to explain *klikushestvo*, showing that their approaches mostly revealed their particular notions of gender, science, and politics. But she also looks at demon possession from the other side. One especially fascinating chapter explores popular conceptions of the Russian Orthodox faith and includes a statistical evaluation of some eighty cases of witchcraft that occurred between 1870 and 1900. Here, Worobec links witchcraft to the tremendous strains on peasants, especially peasant women, after the 1861 emancipation of the serfs. She also compares witchcraft in Russia to witchcraft in Western Europe and even Ukraine. Although she acknowledges that differences abounded, Worobec points out that everywhere witchcraft helped peasants account for hardship, just as it enabled them to punish perceived deviants and reestablish order within their communities. As in the West, the notion of

witchcraft also dovetailed with prevailing beliefs that women were sexually dangerous and susceptible to malevolent forces.

Upon reading this book, one cannot help but sympathize not only with the agonies of the possessed but also with the poor observers who had to make sense of incidents like the one in 1893, when a woman tried to burn the demons out of her eighteen-year-old daughter by stuffing burning thistle into the girl's mouth and vagina. The fact that *klikushestvo* has reemerged in present-day Russia suggests that its historical meaning, function, and origins have contemporary relevance. Its meaning is fairly easy to see: *klikushestvo* served as a convenient trope from which to hang various preconceptions and ambitions. Its function is also within reach: Worobec reveals how the possessed usually received sympathy from their community and exemptions from the relentless obligations of peasant labor. But its origins are trickier. In her concluding chapter, Worobec lists explanations that she finds unpersuasive: anthropological cultural relativism; psychiatric notions of hysteria; the ill effects of eating spoiled rye (ergotism); and the way that calcium deficiency coupled with stress can trigger fits. Her real explanation is something much more mundane, but also more plausible. As a scholar well versed in Russian peasant culture and history, she suggests that we look for the causes of *klikushestvo* in the dire circumstances of peasants' actual lives.

Worobec's book sits squarely within the genre of recent historical works about late imperial Russia that examine social pathologies like hooliganism, prostitution, and crime less to seek their elusive realities than to illustrate the anxieties and aspirations of the educated elite. *Klikushestvo* works well in this vein, being less significant in and of itself than for the way it was manipulated. But Worobec has also succeeded in teaching us more about peasants, and especially about peasant women. I would have liked to see some consideration of how the traditional watershed in the history of Russia's peasants, the emancipation of the serfs in 1861, affected *klikushestvo*. Worobec gives us some insight into this disjuncture when she looks at elite conceptions, but it would have been useful to deal with the emancipation in another way as well. If misery and popular notions of witchcraft and religion gave rise to possession, then might not *klikushestvo* have been more prevalent and perhaps have taken on other forms when peasant women were subject not only to poverty and patriarchal strictures but also to serfdom? The fact that this does not seem to have been the case could reflect lack of reportage, but perhaps it also suggests what Worobec discovered: that the shocks of modernity took their toll not only in the form of revolution but also psychologically and culturally.

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ANN HIBNER KOBLITZ. *Science, Women and Revolution in Russia*. (Women in Science.) Amsterdam: Harwood Academic. 2000. Pp. xv, 211. \$38.00.

The 1860s were a remarkable period in Russian history. After decades of autocratic control and isolation from European intellectual life, Alexander II encouraged his subjects to dedicate themselves to the improvement of their native land. Young Russians responded to their emperor's plea with particular enthusiasm, flocking to the cities and institutions of higher learning. Students gathered in cafés, classrooms, and crowded apartments to discuss Russia's "burning questions," whose central focus was their country's eternal backwardness vis-à-vis Western Europe.

Ann Hibner Koblitz's new book tells the story of one group of these student activists who were influenced by "a confluence of science, feminism, and social activism" (p. 146). These young women—Sofia Kovalevskaia, Iuliia Lermontova, Nadezhda Suslova, Anna Evreinova, Maria Bokova-Sechenova, and others—fell under the influence of nihilism, a doctrine that combined a belief in the transformative power of science with radical notions of human equality. Their feminism encouraged them to seek out whatever educational opportunities they could find in Russia and, when those were curtailed, in Western Europe. Undaunted by the obstacles placed before them, they went on to become the first women to earn their doctorates in mathematics, medicine, and chemistry. Having achieved academic success, these women scientists attempted to realize their dreams of social activism in a variety of ways, which Koblitz chronicles in her book.

Koblitz's chief concerns are to ascertain the conditions that produced these remarkable women and why they were not succeeded by a second generation of women scientists. To do this, Koblitz provides a clear and well-researched account of the intellectual, social, and political setting that prevailed in Russia in the 1860s. Inspired by a heady mix of science, politics, and activism, these women dedicated themselves to improving the lives of their fellow countrymen through personal sacrifice and hard work. What is remarkable about their story is that they were not unique; there were other women in Europe who also dreamed of a richer life, but who failed to achieve their goals. Koblitz argues that this generation of Russian women succeeded due to a remarkable spirit of collaboration and cooperation that was born in Russia and that they carried with them to Western Europe. She analyzes the collaborative relationships these women had with their mentors who encouraged them in their studies and who often shielded them from those who wished to destroy them professionally. At the same time, these women formed a supportive network among themselves. They lived and worked together, providing moral and logistical support at critical periods in their lives. This incredible success story did not survive for very long. The spirit of collaboration and cooperation

that prevailed in the 1860s and 1870s gave way to one that fostered skepticism of women scientists. The cultural milieu in Russia that had produced these women changed as the government grew more suspicious of idealistic students and they, in turn, became disillusioned as their attempts to overcome Russian backwardness were rejected. In addition, Koblitz uses the posthumous reputation of Kovalevskaia to demonstrate how the mathematical establishment denigrated her accomplishments by circulating half-truths about her professional accomplishments and personal life. Given this barrage of criticism and scrutiny, neither she nor any other women scientists of the 1860s served to inspire younger women to follow their example.

In her last chapter, Koblitz attempts to critique those scholars who have combined gender and science theory such as Evelyn Fox Keller and Leanna Standish, using the Russian women scientists of the 1860s as a corrective to their views. This discussion presumes a fairly close reading of these gender theorists' works in order to appreciate fully Koblitz's critique. Since the book is intended for both a scholarly and general audience, this may present problems for readers who are new to the subject. But, perhaps, more importantly, while many of us believe that women are quite capable of achieving excellence in the sciences, they still face very serious obstacles. The spirit of collaboration and cooperation that created and nurtured the women of the 1860s has proven very hard to sustain in the competitive environment of the modern scientific establishment. However, Koblitz is successful in rescuing these women scientists from obscurity so that they may serve as models to future generations of females interested in scientific pursuits.

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RICHARD MOWBRAY HAYWOOD. *Russia Enters the Railway Age, 1842–1855*. (East European Monographs, number 493.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1998. Pp. xxvi, 635. \$91.00.

This volume is a welcome sequel to the author's earlier work, *The Beginnings of Railway Development in Russia in the Reign of Nicholas I, 1835–1842* (1969). In the more recent volume, Richard Mowbray Haywood takes the reader through a wealth of material concerning virtually every aspect of railroad building under the reign of Nicholas I after 1842 until his death in 1855. The tsar, very much the director of railroad enterprises, should be known as the Engineer of the Russian Empire along with his more common sobriquet, Gendarme of Europe. As this detailed study shows, Russian railroads, at least until the middle of the nineteenth century, were the pet project of the autocrat. Any assessment of Nicholas I must now include an appreciation of his achievement of building the largest railway in the world under one administration according to the highest standards possible at the time. As

Haywood illustrates with clarity and precision, in Russia the vicissitudes were many and the obstacles staggering in undertaking to build 403 miles of double tracks from St. Petersburg to Moscow.

Divided into five chronological chapters and an epilogue, each unit of the book contains a number of modules, so that, for example, a reader interested in engineering aspects can easily follow that thread by way of the headings. Even engineering is subdivided into the building of bridges, stations, earthworks, locomotives, tracks, and the telegraph. Those interested in the financing of the railroads can select those sections for perusal. Historians of administrative procedures can easily extract what they wish to learn. Haywood makes telling comparisons of the Russian experience with railroad building in the United States and Europe.

Although they were few, American engineers and mechanics were crucial to fulfilling the tsar's dream. George Washington Whistler, father of the artist James Whistler, receives his share of fame in this book as a trusted advisor to Nicholas on the design of bridges and many other aspects of building the railroad. The American firm of Harrison, Winans, and Eastwick was hired to procure needed materials from England and to design and build car trucks, arches, freight and passenger cars, locomotives, steam engines, switches, and even rockets for the military. The firm also received a lucrative contract to maintain the equipment once the railroad opened in 1851. Reluctant to remain years longer than anticipated in Russia, those American engineers who survived the harsh climate returned to America as rich men.

While Nicholas was eager to draw on American and European technology and even to float loans abroad for needed funds, he controlled every aspect of the project through his Russian subordinates. Most of the engineers and technicians were Russian as well as the thousands of serfs and freely hired labor that toiled under the most horrendous conditions constructing earthworks. Nicholas also authorized the building of the St. Petersburg-Warsaw Railway that was not to be completed until 1862. Unlike Nicholas, who eschewed private enterprise, his son and successor, Alexander II, solicited domestic and foreign entrepreneurs to carry on the extension of the Russian railroad system. In the epilogue, the author discusses various proposals for building other lines such as ones to the Baltic and Black Seas, although none came to fruition under Nicholas I. Perhaps had there been a railway from the capital to the Black Sea in 1853, the outcome of the Crimean War might have been more favorable to Russia.

Haywood has utilized valuable archival materials, rare technical Russian publications, and British consular reports. He has consulted papers, letters, and memoirs in many libraries and historical societies in the United States. Railroad buffs and engineers will be in their element reading this original monograph,

which is accessible even to the non-mechanically inclined.

This is an indispensable work for economic historians of Russia, although social historians will benefit as well. For example, we learn that some of the regulations pertaining to passengers in mid-nineteenth century Russia have a familiar ring. Passengers were to appear an hour before departure to check the bulk of their luggage. They had to show a passport, although initially police permission was also required. No smoking was allowed in any of the cars, not even in first class. Only one piece of carry-on luggage could be brought aboard, and it had to be put under the seat. Lest we perceive Russia as being unusually modern, however, it should be pointed out that there were, at first, no heating in the cars and no toilet facilities, although the stations furnished flush toilets. This is a stout book of solid scholarship; most travelers through its pages will enjoy the trip.

PATRICIA HERLIHY
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ABRAHAM ASCHER. *P. A. Stolypin: The Search for Stability in Late Imperial Russia*. Stanford: Stanford University Press. 2001. Pp. xii, 468. \$55.00.

This book deals with the life and work of Pyotr Stolypin, prime minister of the Russian tsarist government from 1906 until his assassination in 1911. On the one hand, the author, Abraham Ascher is to be congratulated. He has turned out a book about the last years of the Russian tsarist regime, and it does not focus narrowly on the government's political opponents. Most historians in both Russia and Western nations are convinced that street mobs and/or parliamentary debates were the driving force behind most of the significant political developments in Russia during the years prior to the Bolshevik Revolution. Ascher, to his credit, sometimes notes that a government administration was operating and even carrying out active reforms.

Ascher's accomplishment is of no small significance in a scholarly field long dominated by a monumental indifference to the realities of government administration. His work may well set off a new trend in the scholarly investigation of prerevolutionary Russia. Historians of modern Russia may begin to ask meaningful questions about the problems the Bolsheviks encountered after 1917, when they suddenly found themselves trying to govern actively and not merely stirring up crowds. Ascher does not mention the fact, but it was the bureaucrats left over from Stolypin's government who got the "revolutionary" regime running after 1917 and found directions in which it could move.

But Ascher's remarkable achievement is only occasional. Unfortunately, most of his account follows the assumptions of his predecessors. He, too, requires Russian administrators to construct visions of organization around Western fantasies. Following in the

footsteps of would-be historians of Russia since the time of Peter the Great (1689–1725), Ascher strives to answer only one basic question: why have Russian administrators failed to organize themselves around Western fantasies of law and order? What these administrators actually have been doing is of no interest. Whenever tsarist administrators did anything to hold themselves and their people together, it has been generally excluded from what the West deigns to recognize as history. Russia cannot possibly be anything other than a land of evil and/or incompetent leaders and their hapless victims.

The attitude this approach reflects has done much to blind Western scholars to the work that administrators actually do in any organization, be it public or private. Academics who study the openly autocratic governments of Russia have presented an especially grotesque view. It is quite in scholarly order, therefore, for Ascher to stick to the theme on which American pundits like Mark Twain have always rested their thinking about Russia. The Russian government has failed to operate as a perfect system; therefore, it has consistently failed to govern Russia effectively.

There was at least one scholar in tsarist times who abandoned this well-entrenched preconception: Nikolai M. Korkunov, once a professor of state law at St. Petersburg University. Ascher may have read one of this great scholar's books (*Russkoe gosudarstvennoe pravo* [1909]). However, like many of Korkunov's own Russian and German contemporaries (in the 1880s–1890s), he has ignored Korkunov's brilliant insights.

Ascher's indifference to the workings of Russian autocratic administration is clearly revealed in his oracular pronouncement to the effect that it was impossible "to reconcile the principle of autocracy with that of local initiative" (p. 222). In fact, the efforts of Stolypin's government to impose reform on Russian peasant life are one of the best illustrations of a general truth: that the agents of an autocratic government are well fitted by the official vagueness of their authorizations and functions to lead their people along the paths suggested by their own native aspirations. Autocratic agents, answerable only to their superiors, have the flexibility to deal with the obstacles thrown up by clan traditions, parliamentary maneuvering, and the pretensions of systematic bureaucracy. In the latter-day tsarist regime, officials carried at least a few paper weapons that helped them to proceed in the directions that their own interaction with the populace suggested. Stolypin did not "translate . . . abstract ideas on agrarian reform into reality" (as Ascher claims, p. 155), but his government did.

Ascher associates this kind of thinking with the "ultraconservative" (p. 190) Appolon Krivoshein, who, as head of the Peasant Bank and minister of agriculture, actually carried out the highly successful reforms of rural government and the peasantry that have been named after Stolypin. By dismissing the evolution of Krivoshein's rural organization, Ascher forbids us even to try to depict the unique peasant reform that began

under the last tsar. Most of his discussion of rural reform, therefore, is a chorus of stale liberal and revolutionary slogans, cooked up from some of the very oldest preconceptions that have governed the "thinking" of Western experts in Russian history. Ascher has compiled an unusually useful account of Stolypin's life and work, but his view of Stolypin's government is persistently shallow.

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ROBERT SERVICE. *Lenin: A Biography*. Cambridge: Harvard University Press. 2000. Pp. xxv, 561. \$35.00.

No, this biography is not simply a new edition of Robert Service's magisterial three-volume *Lenin, A Political Life* (1985, 1991, 1995). Rather, the biography and the political life together form the most complete, authoritative, knowledgeable, and readable life of V. I. Lenin to date.

The conventional wisdom is that, for Lenin, biography and political life were identical. Service demonstrates quite the opposite. Here is Lenin as if he were a human being, fetishes and all, warm in the cozy conspiracy of his family, outwitting the imperial Russian police at every turn, and organizing the Russian Revolution. To understand the human V. I. Ulyanov (1870–1924), Service read virtually every published source on his subject and explored the newly opened archives of the Russian Center for the Conservation and Study of Documents of Contemporary History: files on Lenin, his wife N. K. Krupskaya, his sisters M. I. Ulyanova and A. I. Ulyanova-Elizarova, his mistress Inessa Armand, Joseph Stalin's library, and Lenin's medical file.

Service's Lenin is an exceptional person, not the one-dimensional psychopath, Jacobin, ideologue, or politician of so many other biographies. Lenin's impact on history was enormous. His personal life thus matters a great deal. Here we see Lenin as a manipulator of women, a hypochondriac, a cyclist, a human time bomb of inner rage, a political warrior, and a fanatical reader and writer. He needed absolute silence to concentrate on his work. He cleaned his desk daily and demanded sharpened pencils. And he suffered from ulcers, migraine headaches, insomnia, and heart attacks.

Lenin came from a provincial middle-class Russian family of mixed non-Russian ancestry, but he always behaved like a Russian. He was also a lifelong professional revolutionary who considered himself the only true Marxist. Yet he read Aristotle, Niccolò Machiavelli, Carl von Clausewitz, Charles Darwin, and Harriet Beecher Stowe. The death of his father and his brother Alexander, a would-be regicide, ostracized Lenin and his entire family socially and drove him to seek revenge against the autocracy and the middle class. At seventeen, he transformed himself from a brilliant student into a revolutionary. His idol was

N. G. Chernyshevsky, the populist radical whose photograph Lenin carried in his wallet.

By the 1890s, Lenin was "determined to bring down the Romanov monarchy" (p. 81) and "the hated Old Russia" (p. 92). He turned to terrorists Sergei Nechaev and P. N. Tkachev for inspiration. His brother Dmitry and his sister Maria were both arrested and exiled for their radicalism. A product of Enlightenment rationalism, Lenin abhorred relativism and became a Marxist theoretician with a lust for violence.

As the "old man" of the Bolsheviks, Lenin in European exile before 1917 was a dogmatic, annoying, resourceful, polemical, and singleminded revolutionary. Fellow Bolsheviks thought him quite mad. *Pravda* rejected his articles. Many Bolsheviks drifted away, leaving Lenin in charge of a dwindling group of increasingly dedicated followers. Lenin also engaged in a long love affair with Inessa Armand, a French-Russian socialist he met in Paris.

World War I gave Lenin his great opportunity. He loudly advocated the defeat of the Russian army in hopes of provoking a civil war. He became a flexible politician of the printed word, more than an orator. In 1917, few Russians knew what Lenin looked like, even though some read his writings. Yet he donned absurd wigs and shaved his beard to disguise himself from the police. Service shows that Lenin was no libertarian in 1917. His *State and Revolution* pamphlet was as hostile to political freedom as his other writings. Lenin helped the Bolsheviks seize power by laying down strategy and inspiring them from behind the scenes.

Service shows us a human Lenin, devoted to his own Marxist vision of revolution and revenge but flexible enough to take advantage of every historical opportunity. Lenin "wished to annihilate every vestige of the old regime and to use every available weapon in the struggle" (p. 322). His ideology, his temperament, and his poor health all drove him toward revolutionary action. Service's Lenin is a multidimensional being who "relished terror" (p. 493) and was personally crude, neat, bookish, intelligent, cynical, odd, and intolerant, a spoiled child dependent on his entourage of women. "The brilliant student who became a gawky Marxist activist and factional leader," concludes Service, "made the most of what History pushed his way" (p. 494). Lenin was a self-assured Marxist theoretician who avenged himself on imperial Russia.

This biography offers the best portrait we have of the man who made the Soviet Union. It helps us understand why the Bolsheviks seized power in Russia, and why the Lenin cult still lives.

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DAVID L. HOFFMANN and YANNI KOTSONIS, editors. *Russian Modernity: Politics, Knowledge, Practices*. New York: St. Martin's. 2000. Pp. viii, 279. \$65.00.

This is a difficult book to review but well worth the effort. The contributors aspire to change our thinking

about "modernity" as applied to Russia. They urge us to think not only or even primarily about institutions, social structures, and material life but about the discourses and practices historical actors used to internalize Russia's relationship to the West. The result is useful and illuminating. Editors David L. Hoffmann and Yanni Kotsonis group the essays under four rubrics: "Toward a Modern Politics" (pre-Reform Russia); "Reform and Revolution as Modern Moments" (1861–1921); "The Paradox of Human Redeemability in Soviet Russia" (1920s); and "Narratives and Identity in the Soviet Context" (1920s–1930s). In the first, Abby M. Shrader and Nathaniel Knight explore social categories. Shrader shows how the authorities used branding, with other instruments of social control, to label criminals, and how they failed to fix their identities. Knight explores, up to the mid-nineteenth century, how *narod* (the people) was used to distinguish the lower classes and how *narodnost'* (the quality of the people) was equated with national identity. In part two, Charles Steinwedel treats ethnicity in the late imperial era and shows, in the government's discourse on Bashkiria, a shift from classification by estate and religious confession to *narodnost'* and *national'nost'* (nationality). Peter Holquist, also writing about linguistic categories and social practice, traces continuities between the revolution and World War I, showing how revolutionaries utilized earlier discourses, as well as practices such as surveillance and allocation of food, "as a mode of power" (p. 91). In part three, Kenneth M. Pinnow argues that doctors who practiced forensic medicine in the 1920s built into their work the assumption, shared with the Bolsheviks, that the social order could be managed scientifically. Contrarily, Francis Bernstein, writing on endocrinology, shows doctors committed to "scientifically verified sexual difference" (p. 139): that is, biological differences, despite the Bolsheviks' insistence on the primacy of social conditioning. Terry Martin, in the last essay in the group, describes the Bolsheviks' failure to promote effectively "a historical and contingent nationalism," so that the government ended up in the 1930s encouraging a "crude form of primordialism" (p. 168). The final section is also more about words than deeds. Thus Frederick C. Corney describes the October Revolution as "less an event than a meaning-making process," Golfo Alexopoulos argues that the victims of Soviet persecutions adopted the language that victimized them," and, last, Jochen Hellbeck describes how the author of a Soviet diary could use the official language to "inscribe himself into the system" (p. 223).

The editors frame these diverse essays with their own. Kotsonis, in his introduction, presents modernity "as a comparative framework," not in order to freeze Russia's position in a continuum but to open up consideration of the various ways in which modernity was expressed in the Russian experience. Hoffmann, in his conclusion, continues this train of thought by suggesting that "Soviet socialism responded to many of

the challenges and aspirations of European modernity," including the urge to reshape society (p. 259). How well have they succeeded in their project? They point up how language was used to reinforce the often-brittle social order even by those undermined by officially accepted categories, such as victims of Stalinist persecutions. Yet they also show the fragility of official categories, as in the case of branded criminals, Soviet forensic doctors who made inflated claims for their profession, and endocrinologists who identified gender with biological differences. Other contributors to the volume highlight the holding power language when linked with practice, as in the case of efforts to shape national identity and to empower of the state during World War I and the revolution. In these ways, the authors have opened up interesting areas and raised important questions.

The reviewer's task is not simply to praise. Popular prerevolutionary Russian writers and journalists, often of common origins, represented national identity, ethnicity, political loyalty, and even the social categories that concern the first three authors. Since I described this nearly two decades ago, I find its neglect irritating. Since then Louise McReynolds, Daniel Brower, and others have also written on this subject, and there is even a volume of translations edited by McReynolds and James von Geldern. Thus Knight, who cites Russia's "weak sense of national cohesion," might have modified his observation had he read popular authors who describe relations between Russians and other nationalities. It matters, for example, that anti-Semitism diminished markedly in cheap commercial publications of the late imperial period, although it persisted in those of the church and state. Similarly, Steinwedel, who argues that "ethnic categories mattered more to political elites who could participate in Imperial institutions than to most of the Tsar's subjects" (p. 80), might have qualified this statement had he had known that Russians and other nationalities, with the exception of Tatars, were shown as having a great affinity for each other in popular newspaper serials. Lastly, in the case of Shrader's essay on branding, potboilers about Siberian criminals disguising themselves illustrate how the problems she discusses reverberated in the popular imagination.

With respect to the Soviet period, several authors could also have touched on a wider context. Thus Pinnow might have noted that other professionals besides forensic specialists defended their professions by making big claims for their expertise. Moreover, since the subject is partly suicide, mention of famous, politically charged suicides might have added a dimension. Similarly, Hellbeck, who argues, using two diaries, that Stalinist subjectivity was "informed by Romanticism" (p. 235), and that individuals sought to realize official versions of the self might have drawn on the theoretical power of the old proverb, "Between what I say and what I do, there is the sea." Czeslaw Milosz, whose *The Captive Mind* (1953) the author could have cited, allowed for such self-deception. Last,

although one can agree with Hoffmann that "attempts to rationalize economic production, categorize the population, and reorder society became increasingly common in Imperial Russia and the Soviet Union" as elsewhere in Europe, these trends were very amorphous. After all, the same could be said of Pol Pot's regime in Cambodia. Yes, Russia shared in pan-European trends, but Europe had many faces.

JEFFREY BROOKS

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DAVID L. RANSEL. *Village Mothers: Three Generations of Change in Russia and Tataria*. (Indiana-Michigan Series in Russian and East European Studies.) Bloomington: Indiana University Press. 2000. Pp. viii, 314. \$39.95.

Russian peasants had perhaps the highest rate of infant mortality in the world, the result, medical experts believed, of the peasants' own practices. Women gave birth with the assistance of "ignorant" peasant midwives. During the summer months, peasant mothers toiled in the fields, leaving infants and toddlers in the care of the elderly or other children. They stuffed their infant's mouth with a pacifier of chewed bread, wrapped in a filthy rag. Such practices, embedded in village women's culture, proved highly resistant to change. Yet transforming village mothering is what the Soviets set out to do. In this book, David L. Ransel assesses their success.

The book is divided into two parts. The first, based on extensive use of published and archival sources, traces the official efforts to stem infant mortality that began in the late seventeenth century. Achievements were modest and came only in the late imperial period, with the introduction of summer nurseries and zemstvo medical care. These vanished in the first decades of Soviet rule, whose impact on village women was largely negative. Assaults on religion, collectivization, the abolition of abortion in 1936, and the dual pressure on women to produce and reproduce, without any assistance from the state, brought enormous hardship. Only after World War II did peasant women gain access to medical facilities and elementary education; summer nurseries arrived more slowly still. In 1955, abortion became legal again, permitting women to control their fertility without risking illegal abortions.

In part two, the focus shifts to peasant women. Drawing upon nearly one hundred oral histories of peasant women, which he and his Russian assistants conducted, Ransel assesses women's response to government policy. Ransel divides his respondents by ethnicity, and into three generations. To generalize more crudely than he does, he finds that the first generation of women, born before 1912, remained largely unaffected. The second, born between 1912 and 1930, was affected, but mainly negatively, while the third, the only generation able to benefit from the regime's innovations, underwent the most marked changes. Most gave birth to their children in state-run

medical facilities, limited their fertility with the assistance of legal abortion, and paid far more careful attention to each child than had their mothers or grandmothers. They became, in short, the most fully Sovietized. But not entirely. As Ransel shows, women adapted selectively, embracing those innovations that seemed beneficial or appropriate, rejecting others of which they disapproved. Their insistence on baptizing children (or performing the equivalent Muslim rite) provides a notable example. Ransel finds comparable generational patterns among his Tatar and Russian informants. The differences are mainly cultural: Tatars nursed infants longer, for example, and they refused to send mothers to the fields.

Ransel's findings are convincing and important. His taped interviews of peasant women, which will be available to others, represent a major contribution to the field. Yet for all its strengths—and they are many—this is in some ways an uneven book. The first part of the book, a careful scholarly analysis of elite attitudes and policy over several centuries, fails fully to connect with the second, in which peasant women recount their experiences of the twentieth century. The purpose of a chapter on changing courtship and marriage practices remains unclear. Ransel provides no historical and cultural context for it, ignores regional differences, and does not follow up on the issues it raises. The anecdotal evidence of the interviews suggests that most marriages were far from satisfactory. Yet he does not explore why women who made individualistic marital choices remained with drunken and abusive husbands, as so many apparently did. To be sure, as Ransel notes, his was a focused agenda: only one of his forty-four interview questions touched on marriage. Still, having introduced the question of marital choice, why let it drop? Some of the sections become repetitive, the result of Ransel's decision to quote extensively from women of different regions, ethnicities, and generations as he traces their fertility choices and attitudes toward birthing, infant death, and child care, among other topics. Finally, I wish Ransel had addressed more fully the impact of his own participation, as a foreigner and a male, on his village respondents, given peasants' well-known propensity to distrust outsiders and withhold from them information. Nevertheless, Ransel has done a major service by capturing these women's voices and narrating a history that is based on their experience. This is an important book that should be of considerable interest to medical historians and historians of peasants, the family, and of women.

BARBARA ALPERN ENGEL

University of Colorado [All reviewers of books by Indiana University faculty are selected with the advice of the Board of Editors.]

CATHERINE MERRIDALE. *Night of Stone: Death and Memory in Twentieth-Century Russia*. New York: Viking. 2001. Pp. vi, 402. \$29.95.

What is the impact of violence on the life of a country, on the daily existence of its citizens, their ability to cope, their productivity, perceptions and viewpoints, fears and aspirations? In the case of twentieth-century Russia, bloodstained as it was for the past hundred years by political brutality, perpetual violence claimed a shocking figure of over 50 million casualties. Catherine Merridale's study examines people's attitudes, rituals, and beliefs associated with death, and few societies merit this inquiry more than Russia, where human fatalities exceed victims of bloodshed anywhere else in the contemporary world.

Since death-related customs are particularly resistant to change, Merridale purports that their analysis may facilitate understanding of certain essential elements of the Russian culture: those that successfully withstood the official Soviet policy of ideological domination. This understanding is possible, however, only if we depart from what Merridale sees as the pernicious, if conventional, tendency to focus on the long tradition of violence as the vital point and a unique cultural feature of the Russian national history. While acknowledging the enormous scale of bloodshed, suffering, and loss in Russia's recent past, Merridale insists that Russian society has not been "distorted by some fault, some freak of custom, culture, or geography," that Russians "are not members of a different humanity" (p. 11).

This argument seems to be the central conceptual point of Merridale's study and simultaneously its principal shortcoming. It hardly requires scholarly insight to disclaim as racist any generalization about twentieth-century Russia affected by unrelenting violence as a sign of some fatal illness or as punishment for its culture being inherently barbaric, primitive, brutal, and forever lacking respect for human life. It would, however, take much discernment to explain why nonetheless brutality did play such a tremendous role in the country's history. For her part, Merridale merely states that the Russian "history of violence did not grow from some national eccentricity, like a taste for eating chunks of salted lard, but from particular combinations of events and circumstances" (p. 11). Yet, if Merridale reduces the determinist argument to "lazy thinking [and] sheer evasiveness" (p. 11), what should one call relegating any in-depth explanation of the central phenomenon of her own inquiry to "particular combinations of events and circumstances"?

Merridale notes but does not explain why so many Russians consider themselves victims of uniquely native, geographically and culturally predetermined violence—a result of the Tatar yoke or perhaps the brutality of the climate. This characteristic bigotry manifests the need for special attention and warrants multifaceted evaluation in the psycho-historical realm, potentially shedding some light on the causes of conventional self-deprecation. Overlooking the need to discern intrinsic or acquired national cultural patterns—including nation-specific attitudes toward the individual's life and death—Merridale reduces her

explanation of "events and circumstances" to political and ideological factors. Yet, since death applies only to the individual (even if people are killed en masse), it would seem essential to understand what it means in the Russian tradition to be the individual, the "self." Merridale states that "in the Soviet Russia, the issue of 'being a person' was complicated by the Bolshevik Revolution, by Communism, scientific atheism," and other sociopolitical factors. Unfortunately, she fails to consider that long before 1917, great many Russians had trouble "being persons," partly due to a centuries-long connection to the commune, the obligatory Orthodox *sobornost'*, and other aspects of collective living. And it might be suggested that a closer look at the way Russians perceive death and mourning should perhaps be taken through the prism of their subdued notion of the self.

Conceptual flaws aside, Merridale's book is a well-researched and well-written work based on a diverse body of data ranging from archival to oral history materials. Indeed, this is a valuable attempt to intertwine official documents and other written sources with spontaneous human voices. There are, however, some questionable and occasionally incorrect assertions, such as the one about Alexander III's government doing little to help the peasants during the 1892 famine (p. 54). The opposite has been well demonstrated by Richard G. Robbins, Jr., in his *Famine in Russia, 1891-1892: The Imperial Government Responds to a Crisis* (1975). There is also sporadic ideological tendentiousness, such as Merridale's claim that "many of the deliberate policies of the Russian ruling class seemed calculated to encourage unnecessary death" (p. 62).

On the whole, however, the reader will be impressed with Merridale's unusual approach to the soul of the people through their responses to death. She must also be congratulated for her courage in writing in her own, very personal voice, imparting her experiences and attitudes, strong emotions, and compassion for the suffering she describes. This openness in presentation is a rare and welcome contribution to academic scholarship presently dominated by somewhat exaggerated preoccupation with impersonal modes of inquiry.

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Boston University

MIDDLE EAST AND NORTHERN AFRICA

MARILYN BOOTH. *May Her Likes Be Multiplied: Biography and Gender Politics in Egypt*. Berkeley and Los Angeles: University of California Press. 2001. Pp. xxxviii, 460. Cloth \$65.00, paper \$35.00.

Biography occupies an exalted place in Islamic letters. Owing to the necessity of verifying the chains of transmitters of the Prophet Muhammad's utterances (*hadith*), the genre emerged very early in Islamic history. The first biographical dictionaries of *hadith* transmitters date to the early ninth century. By the

thirteenth century, these dictionaries had expanded to include eminent scholars in all the Islamic sciences, as well as political leaders.

Marilyn Booth's book examines the deployment of this genre in the construction of modernity in Egypt, and women's role in that construction. She surveys the emergence of a new subgenre of prescriptive biography, of and often by and for women, in Egypt in the late nineteenth and early twentieth centuries, when a nascent Egyptian nationalism and the British occupation occasioned a reexamination of women's public and private roles. Booth's sources consist of "571 biographies published in eighteen magazines focusing on women as writers, subject, and/or audience from 1892 to 1949, plus selected additional biographies from other magazines and collections from 1879 to 1967" (p. xxxv). Added to these are numerous biographies from the contemporary Islamist press.

The true gems here are the wildly disparate "exemplars" that Booth culls from her exhaustive reading: women from throughout the world and throughout history, from Hatshepsut to the Prophet Muhammad's daughter Fatima to Joan of Arc to Catherine the Great to the Egyptian feminist Huda Sha'rawi are marshalled as models worthy of imitation by the modern Egyptian woman. The first four chapters stay largely within the above-mentioned time frame as Booth explores the solidly bourgeois and overwhelmingly domestic virtues that women's magazines doggedly ascribed to these sometimes unlikely role models: thus, Queen Victoria is home educator nonpareil, while the cross-named, cross-dressing George Sand, survivor of numerous affairs, is lauded as "best of exemplars in her morals and conduct" (p. 70). In chapter five, the author's argument gains chronological momentum as she explicates the change in the presentation of women's lives from the 1890s, when publicly active, if unassailably domestic, women served as global exemplars, to the 1930s, when the homemaker-cum-tutor surfaced as the preeminent female role in the service of an invigorated Egyptian nationalism. Booth then unpacks the image of Joan of Arc as quintessential anti-British peasant nationalist during the interwar years, when the peasant was emerging as the symbolic backbone of Egyptian nationalism, before turning to the following decades, when photographs and scandals seem to displace sober lessons of domesticity as the new currency of women's journalism. This leads into a concluding consideration of present-day Islamist constructions of the ideal woman, in which Western exemplars are foresworn in favor of timeless appeals to the mothers, wives, and sisters of early Islamic heroes.

The book is structured according to themes stressed within biographies; Booth never explicitly addresses such external issues as the frequency with which such life histories were published or the arrangement of biographies in a series. Although chapter two does attempt to gauge the relative space accorded to the biographies of Muslims, Arab Christians, and Westerners (Americans and Europeans), there is little

attention to how these biographies are distributed in the magazines examined. A biographical series in which, for example, the lives of five famous European women precede that of the Prophet's mother would convey a very different impression from one in which five female companions of the Prophet are followed by one contemporary European woman. Likewise unaddressed, although occasionally alluded to, is the question of how these modern biographies compare to the "traditional" genre of biography-cum-obituary that reached its epitome during the Middle Ages; in particular, what were the implications of devoting biographical space to living women instead? Similarly, were lives of exemplary women popular reading in other developing countries, or in other British-ruled territories during the same era? For comparative purposes, curiously, the author seems to privilege studies of Western European and American women.

This book seems designed to appeal to a broad audience; even the chapter titles are clever and eye catching. The text, however, is replete with postmodernist jargon, at a few points approaching self-caricature. This element might make it less than accessible to a wide readership. Notwithstanding, it is a worthy addition to the growing literature on women's magazines, as well as that on modernity, both in the Middle East and elsewhere, and it will be read with profit by specialists in women's studies and scholars of the modern Middle East.

JANE HATHAWAY
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TORRE T. PETERSEN. *The Middle East between the Great Powers: Anglo-American Conflict and Cooperation, 1952-7*. New York: St. Martin's. 2000. Pp. xiii, 170. \$65.00.

Norwegian scholar Tore T. Petersen argues in this slim volume that "there was nothing inevitable" about Britain's decline in the Middle East over the years 1952-1957. (Scholars generally would agree, but given conditions after World War II and the intensification of nationalism in the region, which Petersen barely acknowledges, London's prospects in most of the region were decidedly unfavorable.) To substantiate his claim, he examines three important crises during those years, in Egypt, Iran, and the Buraimi Oasis. The first two are reasonably well known, the third distinctly less so.

According to Petersen, the Buraimi Oasis crisis deserves our attention, for it was more than a minor incident in Anglo-American relations. The oil-rich oasis, whose ownership was in dispute, lay along the troubled border between an expansionist Saudi Arabia, home of American-owned Aramco, and several Persian Gulf sheikdoms that looked to Britain for support. Tensions there adversely affected relations between Washington and London. Petersen views the British reoccupation of Buraimi in 1955 as a turning point, a successful application of military force, which

led directly to similar action against Egypt the following year. Unfortunately, he devotes too few pages to this issue. Instead, arguing that there have been too many single-country studies that treat their subjects in isolation, he proposes to show the interconnectedness of regional events. Although there is some justification for the observation, Petersen would have done well to focus on Buraimi, which is the most original part of his study. Interconnections could have come later or perhaps in a longer study. This book is much too brief to accomplish the ambitious objectives the author sets out.

Petersen claims three goals: to "give a different perspective," "to give a different interpretation of events than that of most scholars," and to present new material with regard to Buraimi and the Arab peninsula (p. xiii). He most nearly succeeds with the third objective. As for the other two, he criticizes the works of several scholars, chiding each in turn for overlooking some important issue or for some mistaken interpretation (pp. xii, 67-69). In making such challenges, it would seem incumbent that he present his evidence methodically, proving that his interpretations are more accurate, but he does nothing of the sort, leaving the reader unconvinced and surprised at his temerity. In spite of all the ballyhoo, Petersen's views do not differ substantially from those of other scholars.

Just a word about the discussion on the crisis in Iran: the British blockade had not bankrupted Iran (p. 19); the United States wanted to send technicians to Kermanshah, not Abadan (p. 21); Herbert Hoover, Jr. did not go directly to Tehran but stopped in Amsterdam to meet with representatives of the Anglo-Iranian Oil Company (AIOC) (p. 25); the Eisenhower administration organized the coup against Musaddiq, not because it wanted to support British policies but rather because, in its view, Musaddiq posed a threat to stability in the region and might become an easy target for a communist takeover (p. 69). These errors—with exception of the last—are not in themselves of great significance, but they do reveal the challenge of writing broad regional studies and show why most scholars begin with something less complex.

Petersen is on more solid ground when he emphasizes the centrality of the Middle East to understanding U.S.-British relations in the 1950s and also when he sees cooperation as well as competition during those critical years. I would agree that Britain became an effective "junior partner" after 1956, in contrast to France, which insisted on going its own way. (His discussion of this latter development is insightful but once again all too brief.) None of these points, however, represents the promised breakthrough in interpretation.

Regretfully, in the first half of the book, hardly a page is free of typographical errors, grammar mistakes, and malapropisms; even the frequent lengthy quotations have their share. Authors, of course, have the responsibility to ensure that their works are properly proofread, but I could not help wondering how

many mistakes would be too many for this press. Although this study fails to achieve all of its goals, it is to be hoped that Petersen will continue his research on the Middle East, an area that is poorly represented by scholars in this field, and that his subsequent work will move beyond these shortcomings.

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SUB-SAHARAN AFRICA

T. C. McCASKIE. *Asante Identities: History and Modernity in an African Village 1850–1950*. (International African Library, number 25.) Edinburgh: Edinburgh University Press, for the International African Institute, London. Indiana University Press. 2000. Pp. ix, 277. Cloth \$49.95, paper \$22.95.

This book by T. C. McCaskie is a social history that recaptures a century (1850–1950) of ordinary lives of the people of Adeebaba, a village in the West African country of Ghana. A number of factors influenced McCaskie's selection of this small Asante community as the focus of a major study. Adeebaba village was founded in the 1840s by one Kwasi Brantuo. A servant of proven devotion to the king, Kwaku Dua I (r. 1834–1867), Brantuo reached a position of prominence as head of the *Manwere fekuo*. Members of the *Manwere* served as ears and eyes of the king, and they were charged with “punishing wrongdoing” (p. 33). To symbolize his wealth and rise to power, Brantuo acquired land at Adeebaba and settled it with people (pp. 40–57).

The relationship between the Asante state (the Golden Stool) and its men of means was the subject of Ivor Wilks's essay, “The Golden Stool and the Elephant Tail: An Essay on Wealth in Asante,” published in George Dalton's edited collection *Research in Economic Anthropology* (1979). In separate essays, McCaskie has discussed the specific wealth and history of the *Manwere* division. His focus here must be a walk through familiar territory. More important, this same village was among the few communities in Asante where the eminent British social anthropologist, the late Meyer Fortes, and his colleagues gathered research information that became the Asante Social Survey (ASS). The University of Birmingham in the United Kingdom is the new home of Fortes's ASS materials. With the availability of such important field notes on Asante social lives, in addition to court materials from the Royal Manhyia Records Office, McCaskie is able to present an excellent social history framed by the political developments of the time.

The major political events of the period under discussion include the fact that Brantuo's rise to wealth and power occurred as a political favor from Kwaku Dua I. With Brantuo's death in 1865 and that of his benefactor in 1867 (p. 58), the fortunes of the village and people of Adeebaba changed. From the mid-1860s through the end of the nineteenth century,

the power and fortunes of the *Manwere* division fluctuated. In 1872, for example, then-king Kofi Kakari (1867–1874) ordered the execution of *Manwerehene* Kyei Kumaa and stripped the various offices that composed the *Manwere* division of their material power (pp. 58–62). Also, during the first British invasion of Kumase in 1874–1875 and again in the period of the Asante civil war of the 1880s, the land and farms of Adeebaba were ransacked by troops. But while these developments hindered the village's opportunity to realize its potential as a settlement equal in stature to its founder, its survival is explained by McCaskie in terms of the power and recognition of *Taa Kwabena*, an important shrine that manifested itself there in the 1870s.

The author's use of records—the memories and stories of ordinary people—to capture developments is classic. McCaskie agreed with R. S. Rattary and Fortes that the traditional family and the village were the key units of social and political structure in Asante (p. 3). But, more importantly, Adeebaba's proximity to Kumase, the availability of ASS information on such sites as Asokore and Effiduase, and the existence of rich Asante historiographical information made it possible for the author to engage in quantitative and qualitative analysis even in situations where the material on Adeebaba alone would have been scant.

Central to the discussion is the subject of continuity and change as expressed in the stresses and aspirations of the people of Adeebaba. A major source of uncertainty was the British exile of the king of Asante and some of his leading advisors following their defeat of Asante in 1896. This marked the beginning of the British colonization of Asante and, from there on, Kumase became the organizing center of the colonial economy for Asante and its immediate surroundings.

While the colonial presence intimidated some, it also offered opportunities to others. The extension of the railway system from the coast to Kumase in 1903, the introduction of the colonial currency as medium of exchange, and the overall cash economy of the colonial period had broader ramifications. Young men traveled afar to mining towns in search of employment. Kumase, the colonial administrative center of Asante, also became a market town for the surrounding villages. Colonial improvements in Kumase—such as the introduction of electricity, the establishment of big European trading stores, the opening of night clubs, the inflow of free persons of non-Asante origins, and even the 1945 “Draft Town Planning Scheme” of the city by Fry & Drew—all impacted relations even in Adeebaba village. McCaskie explores the rich diversity of testimonies to develop a broader understanding of Asante identities. By this strategy, he is able to achieve his goal of presenting “a microhistorical texture that complicated together empirical density with analytical rigour” (p. 238). This is an excellent social history, not only of the village of Adeebaba but also of the city of

Kumase. No future history of Kumase can be written without reference to McCaskie's book.

DAVID OWUSU-ANSAH
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ANDREAS ECKERT. *Grundbesitz, Landkonflikte und kolonialer Wandel: Douala 1880 bis 1960*. (Beiträge zur Kolonial- und Überseegeschichte, number 70.) Stuttgart: Franz Steiner. 1999. Pp. x, 503.

Douala, a modern city of over one million inhabitants is the largest city in the present-day Republic of Cameroon. It is situated four degrees north of the equator on the Cameroon estuary at the mouth of the Wouri River, about twenty miles upstream from the Bight of Biafra on the west coast of Africa. In this outstanding book, a revision of his doctoral dissertation, Andreas Eckert traces in detail the development of land ownership policies and land disputes from the precolonial period, through the German protectorate (*Schutzgebiet*) and the French occupation under the mandate of the League of Nations and the United Nations, until its independence. He develops in rich detail how the people of Douala changed during this time from "*Menschen des Wassers*" (people of the water) to "*Menschen der Erde*" (people of the land).

Germany obtained a protectorate over the Cameroon territory on July 16, 1884 as a result of a treaty signed between King Bell and King Akwa with Gustav Nachtigal (1834–1885), who had been sent there on a special mission by Chancellor Otto von Bismarck. At that time, Douala consisted of a number of trading stations and towns such as Cameroon, Bell, Akwa, and Hickory. The Germans set out to develop the interior of the country as well as combining existing towns into one large city. From Hickory, a railroad was built into the interior to collect rubber, palm products, copra, and cocoa that had been introduced on German plantations. These and other products were then shipped to Germany.

In this engaging study, Eckert greatly enriches our understanding of the transformation of land ownership in Douala. In precolonial times, the right to land was the right to use it; no other right existed, nor did it occur to the Douala to claim one. Once the Europeans arrived, the European concept of land rights was introduced and tremendous changes resulted. Eckert examines the transformation of land rights on four planes: legal, economic, political, social, and cultural. The city of Douala was selected because it was in the urban setting, according to the author, that the encounter between local people and the Europeans was the most intensive and the influence of the European legal norms was the greatest. Douala, as evidenced by the extensive archival sources used, provided sufficient source material to undertake a study of the connection between land ownership and land disputes, as well as the economic, political, and social changes that resulted. In the eighty years under study, a conglomeration of villages was transformed into a modern metrop-

olis that became the economic and administrative center of the country. At the same time, the Douala, who prized education, were transformed from middlemen in the Atlantic trade to professionals and administrators of the city.

Eckert traces out in detail the decline of the local people as middlemen and cash crop producers as the ownership of land changes over to the European concept of land tenure. During the German period, parts of Douala were rebuilt with wide streets, modern housing, sanitary facilities, and a free cut of vegetation was instituted around the city to control malaria. After World War II, additional improvements were made by the French as a result of FIDES (Fond d'investissement et développement économique et social); harbor facilities were improved, and a 2,200-meter bridge was built across the Wouri River. Sanitary conditions and housing were improved under the direction of the architect Henri Calsai. More of the Douala adopted the French concept of land tenure. By the time they achieved their independence, the process of land change was nearly complete and the Douala were no longer middlemen but the professionals and administrators in the city.

Eckert sustains his thesis throughout as a result of research in the archives of Germany, France, England, and Douala as well as by undertaking many interviews in the city. The book's 143 pages of statistical tables are a treasure trove for future researchers. Two works that should be read along with this one are Maria-Theresia Schwarz, "*je weniger Africa desto besser*": *Das deutsche Kolonialkritik am Ende des 19. Jahrhunderts; Eine Untersuchung zur kolonialen Haltung von Linksliberalismus und Sozialdemokratie* (1999), which details the scandals in connection with rebellion in the Cameroon under German administration and how it was reported in the German press and debated in the Reichstag; and Ralph Austen and Jonathan Derrick, *Middlemen of the Cameroons Rivers: The Duala and their Hinterland c. 1600-c. 1960* (1999), which concentrates on the Douala as an ethnic group and covers, although in less detail, some of the same material as Eckert.

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TOYIN FALOLA. *Nationalism and African Intellectuals*. Rochester, N.Y.: University of Rochester Press. 2001. Pp. xx, 372. \$75.00.

Toyin Falola is the doyen of African historians working in North American universities. (His new book contains some stimulating reflections on the relationships between such African scholars and Afro-American Afrocentrists). He is the author of several books on the history of the Yoruba of Nigeria and in particular on the Yoruba intellectuals who from the nineteenth century onward have produced a rich "national" historiography. In this new book, he sets out to widen his

focus and to study African intellectuals and African nationalism as a whole.

Falola defines African intellectuals as those men (and a few women) who obtained advanced Western educations. He points out that intellectuals so defined existed before formal colonial rule. But he does not explore Islamic ideas nor extend his definition to "organic intellectuals" in the African countryside. Steven Feierman's remarkable study of Tanzanian peasant intellectuals does not appear in his bibliography. Still, his definition leaves him with plenty of ground to cover. In successive sections he examines the response of African intellectuals to the problem of "modernity," to the concept of "nationalism," and to the idea of Pan-Africanism. In a conclusion, he discusses what the coming of national independence has meant for African intellectuals, describing the "rise and decline of the African Academy" and reviewing "nationalist historiography." In each section, he identifies the key thinkers, providing brief biographies and accounts of the development of their ideas. The book has an encyclopedic feel to it, and there is no doubt that it will provide the best available guide to nationalist intellectuals from their hopeful emergence in the nineteenth century to their disillusioned embitterment at the start of the twenty-first.

Nevertheless, I have some reservations. It is inevitable in a book that privileges precolonial intellectuals that there should be a heavy emphasis on West Africa. Falola writes: "As my analysis unfolds, intellectual heroes such as Bishop Ajayi Crowther, the Reverend Samuel Johnson, Edward Blyden, Mojola Agbebi, Bishop James Johnson, and others, will feature prominently." They certainly do, as do, later, Nnamde Azikwe, Kwame Nkrumah, Alioune Diop, and many others. It might be objected that the encyclopedic character of the discussion of these men—one brief life after another—gets in the way of large overall analysis. I think, though, that intellectual nationalism can only be defined as the composite of the ideas of these men and many others, with all the inconsistencies, contradictions, and irregularities that result. What worries me is not so much that there are too many men (and a few women) but that there are not enough.

In particular, South African intellectuals hardly figure at all. It is true that nineteenth-century South African intellectuals were not "pre-colonial"; but it is equally true that they had ideas about modernity, about nationalism, and about Pan-Africanism. A figure like Sol Plaatje, for example, really should be present in a discussion of this sort. It is also true that there is a contrast between intellectual continuities in West Africa, so that present-day Nigerian historians are recognizably in a line of descent from Samuel Johnson and other early Yoruba scholars, and intellectual discontinuities in South Africa, where there are hardly any black university historians at all. To explore this contrast might add some more ambiguity to the concepts of modernity and nationalism.

I also think that there is not enough in this book about politics and power and the realities of the nationalist takeover and of nationalist government. Falola calls his intellectuals "powerful yet powerless" (p. xx). He assumes that they created concepts that were then imperfectly realized by many nonintellectuals, and that once nation-states had been created they could dispense with the "powerless" intellectuals, who nevertheless longed for power. I am beginning, I realize, to make unfair demands on Falola. A book cannot include everything. But I think it would have been useful and interesting to have a section inserted between the discussion of intellectuals and ideas of modernity, and the discussion of intellectuals under the nation-state. This section would look at what happens when intellectuals themselves have the power to try to carry out their ideas.

It seems to me that Tanzania's Julius Nyerere, *Mwalimu* or teacher, would make the ideal case study. Long ago, Ali Mazrui warned "Tanzaphiliacs" of the dangers of being ruled by even the most kindly headmaster, a man who would eventually lose patience with his pupils' halting attempts to learn their practical and ideological lessons and who would himself impose a master plan. Nyerere's Tanzania is an example not of what happens when intellectuals are packed away into their box by ruthless practical men but of what can go wrong when ideas rule. It is a shock to find Nyerere's Tanzania in James C. Scott's book on the mind of the modern state as a case study, along with Prussian forestry and Stalinist collectivization. (Indeed, Scott himself stresses Nyerere's benignity.) But the comparison is in this context apt. Nyerere was certainly thinking about "modernity" and acting out his thoughts.

I do not want to end with an emphasis on what has been left out of this book. There is much to enjoy in it. It is valuable that Falola should assert that, despite all the ensuing disillusionment, the coming of national independence to Africa was a great achievement. He is right to say that African intellectuals should not engage in fashionable theories just for the sake of international respectability, but only if they provide weapons for attacking the great questions that concern Africa, even if in many cases these remain "the same old questions." Relevance is more important than novelty. As I have argued for Zimbabwe, one cannot move wholesale to a "post-nationalist" problematic, when the key debate in contemporary politics remains the debate over the nature and history of the nation. I am sending my review copy of Falola straight off to the Department of History at the University of Zimbabwe. They will find it invaluable in their historiography course.

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CLIVE GLASER. *Bo-Tsotsi: The Youth Gangs of Soweto, 1935–1976*. (Social History of Africa.) Portsmouth:

Heinemann. 2000. Pp. xvi, 214. Cloth \$65.00, paper \$24.95.

In post-apartheid South Africa, historians are catching up with ordinary women. As the "crime wave" became democratized along with the polity, so thugs became of more import to intellectuals. Black men whose classrooms were the prisons, whose business was rape and robbery, whose heroes were men who stabbed (and superstars those who murdered), and whose status symbols were jail sentences have long threatened black women, and they are now being earnestly pursued by academics.

This book by Clive Glaser addresses some forty years of juvenile delinquency in South Africa's major urban conglomeration. It focuses not on the migrant worker gangs that have attracted most historiographical attention but on those formed by unemployed, fully urbanized African youths. Glaser's argument can be briefly stated: "young ghettoized males had a real chance of acquiring prestige" through gangs (p. 71). The broader socioeconomic context of unemployment and poverty is the backdrop; culture, and relationship to political movements, are center stage.

Culture, argues Glaser, particularly American gangland culture gleaned from films, was central. After permanent urbanization accelerated in the 1930s, a name was accorded to city slicker African youths: "*tsotsi*." This originally referred to skintight "zooty" trousers known as "zoot suits" in America. Only later did youths wearing *tsotsis* become synonymous with unemployed thugs who were *tsotsis*. Glaser continues to explore the American lodestar by which these lumpenproletarian youths steered, as well as their dress and consumption patterns, movie and sporting interests, language, and rituals (such as burning corpses with benzine.) Gangs ranging from boys' street-corner networks to those of big-time professional criminals are discussed.

In the main, the criminality of these semiliterate youths was incompatible with the politics of an educated elite. Nonetheless, at particular moments, convergence occurred. In what are perhaps the best chapters, Glaser traces gang involvement in the masculinist anti-pass campaign of the Pan-Africanist Congress in 1959–1960 and in the Black Consciousness movement and Soweto uprising of 1976. The book ends by noting that while gangsterism was temporarily

eclipsed by the militant youth subculture of late apartheid years, resurgence occurred thereafter.

The book is primarily descriptive rather than analytical. It is also gender biased. Women are, as Glaser expresses it, "peripheral" in a book "concentrating on the world of men" (p. 6). This is deeply damaging: Glaser himself, and others, have already written about the centrality of women in urban gangland. Whether as the single mothers of *tsotsis*, or as the booty over which fighting constantly erupted and over which male control was brutally exerted, women are the *sine qua non* for violent machismo. Glaser claims that rendering women peripheral does not ignore gender, since masculinity is explored. Yet the notion that masculinity can be examined by focusing on men is the equivalent of the idea that racism can be analyzed by focusing on whites.

There are other problems. Focusing on how muggers walked, talked, and dressed is a way of making them less alien to middle-class readers in university seminar rooms. Yet for those (the great majority) who risked being at the wrong end of the gang rape or the gun, institutions (like prisons) that bred thuggish male criminality are perhaps of more import than stovepipe pants. Glaser's social and geographical focus is narrow, with the countryside virtually nonexistent. Yet asserting an Americanized urban style required an "other": the African rural and migrant lifestyles that were contemptuously rejected. Finally, there is the problem of genre. The text is framed by an breathless epigraph: an academic studying *tsotsis* would be able to "write a thesis that will excite many universities into honouring you with degrees!" Yet just as *tsotsi* and elite cultures diverged, so the lives of astonishingly violent thugs are not necessarily best represented by university theses turned into books. The most telling images in this text are visual. Particularly memorable, in a book with no index entry for women, is a photograph of a young black teenager with a huge "Z" carved on her forehead. She was once one of the many chattels of a gangster called Zorro, a martial arts expert, who branded his women for life. Glaser has noted this elsewhere, and it is unfortunate that his progress up the academic ladder has simultaneously been one of learning to put women into the pictures and out of the text.

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Film Reviews

BRIDE OF THE WIND. Produced by Lawrence Levy and Evzen Kolar; directed by Bruce Beresford; written by Marilyn Levy. 2001; color; 99 minutes. Distributed by Paramount.

Inasmuch as the life and loves of Alma Mahler (1879–1964) seem more like overheated Hollywood melodrama than real life, it is surprising that only two motion pictures so far have taken up the subject: Ken Russell in *Mahler* (1973) and now Bruce Beresford in *Bride of the Wind* (2001). Both filmmakers duly regard the redoubtable Alma with mingled astonishment, awe, and veiled disapproval. In her eighty-five years, Alma survived two world wars, three celebrity marriages (to composer Gustav Mahler, architect Walter Gropius, and dramatist/novelist Franz Werfel), numerous affairs (with composer Alexander Zemlinsky, dramatist Gerhardt Hauptmann, pianist Ossip Gabrilowitsch, and painters Gustav Klimt and Oskar Kokoschka, among others), the deaths of two of her children, and several abortions. She traversed with aplomb the worlds of *fin-de-siècle* Vienna, war-torn Europe, and Hollywood. Although her creative endeavors were modest—she willingly stifled her own composing out of deference to Mahler’s music—she did provoke or inspire some of the twentieth century’s greatest masterpieces, including Mahler’s Sixth and Eighth Symphonies (1906 and 1909, respectively), Kokoschka’s monumental painting *Bride of the Wind* (1914), and Alban Berg’s opera, *Wozzeck* (1922). Satirist Tom Lehrer even wrote a song, “Alma,” about her.

If the portraits of Alma screened in the Russell and Beresford films vary greatly—in Russell’s film, as portrayed by Georgina Hale, she is a rosy-cheeked child in braids who turns into a cuckolding stripteaser; in Beresford’s new work, as played by Sarah Wynter, she appears as a poised, hothouse beauty who soars phoenix-like above the wreckage of her men—we have only Alma herself to blame. Her two volumes of memoirs—*Gustav Mahler: Memoires and Letters* (1946) and (with E. B. Ahston) *And the Bridge is Love* (1958; a German version, under the title *Mein Leben*, appeared in 1960)—were self-serving documents that rearranged or simply invented the circumstances and details of her life. Only recently have her diaries and

newly found caches of letters revealed the truth of a woman plagued with indecision, confusion, self-doubt, uncontrollable erotic fantasies, and occasionally heartless pronouncements toward men.

Bride of the Wind begins in 1902, when the nineteen-year-old Alma Schindler strides confidently into a glittering ballroom, her slim sheath of a dress glowing crimson in the otherwise black-and-white frame. It is a great moment. But once color bleeds into the film (thanks to the ingenuity of the great cinematographer Peter James), the life seems to seep out of it. The story settles into a series of vignettes characteristic of most conventional composer biopics. In addition to the requisite series of “great moments in music” (including excerpts from Mahler’s Third, Fifth, and Eighth Symphonies and a couple of songs by Alma), there are: the first meeting in 1902, as Alma and Mahler lock eyes (actually, that moment occurred several years before); the romantic clinch, set in a half-lit bedroom; Alma’s *Götterdämmerung* as a stifled artist; the tragic loss of their five-year old child, Marie; the act of betrayal, when the distraught Alma begins a clandestine affair with Gropius at a health spa; the confrontation scene between the lovelorn Gropius and the jealous Mahler; the artist’s triumph, in Mahler’s final Carnegie Hall concerts in New York City in 1910; and Mahler’s death and apotheosis a year later. The film ends in 1918, as Alma, fleeing from an unpleasant affair with Kokoschka and entering into another with Werfel, bows to the applause that follows a concert of her music.

But wait: Alma’s story has forty-six more years to go, years of tumult and adventure that include Alma’s hasty flight to freedom across the Pyrenees in 1938, just ahead of the invading Nazis, and her subsequent sojourns in Hollywood and New York City. *Bride of the Wind*, either because of budgetary shortfalls, time constraints, or just plain weariness on the part of the filmmakers, encompasses all that in a brief, concluding slide show of images and explanatory title cards, including the perhaps irrelevant assurance (in this context at least) that Mahler’s music “is now performed and recorded as frequently as that of Beethoven, Bach, or Mozart.” (Yes, one wonders, but what about Alma?) Small wonder that the film enjoyed a limited theatrical run and incurred some disastrous critical notices. What eludes many critics and perhaps

the casual viewer, however, is the real significance of Alma's confounding passivity and elusive personality.

Rather than choosing just one Alma persona to portray, *Bride of the Wind* chooses many Almas, as many Almas as there were men in her life. She embodies a somewhat late version of a classic nineteenth century phenomenon, a "type," one of those spiritualists, actresses, artist's models, dilettantes, and demimondaines that Sheldon Norman Grebstein has called the "Victorian Vamp" ("Dreiser's Victorian Vamp," *American Studies* [Spring 1963]: 3–11). They appeared at the time on stage as Sarah Bernhardt, in the paintings of Edvard Munch, and in novels like Emile Zola's *Nana* (1880), Theodore Dreiser's *Sister Carrie* (1900), Henry James's *The Bostonians* (1886), and Edith Wharton's *The House of Mirth* (1905), and they figured latterly in the screen personae of Marlene Dietrich and Greta Garbo.

This "Alma" is nebulous and vague because, like her mother (a professional actress), she enacted with breathless variety and virtuosity the "roles," the fantasy projections, of her lovers. Her real talent was not creative but imitative. She excelled in what historian Bram Dijkstra describes as "the characteristic feminine imitative capacity of 'reflecting' the world in which [she] lived without ever really understanding its deeper meaning, its intellectual dimension" (*Idols of Perversity* [1986], p. 121). Thus, for Mahler, she was the maternal secretary and muse; for Kokoschka, the erotic model and dangerous lover; for Werfel, the creative artist. Dissatisfied with choosing just one kind of "Alma" to depict, *Bride of the Wind* chooses many. As Alma did herself.

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CONSPIRACY. Produced by Nick Gillott; directed by Frank Pierson; screenplay by Loring Mandel. 2001; color; running time 96 minutes. Distributed by Home Box Office (HBO).

Conspiracy is a docudrama about the infamous Wannsee Conference of January 20, 1942, at which Nazi officials discussed implementation of the "Final Solution." Chaired by Reinhard Heydrich, head of the Reich Security Main Office (RSHA), the meeting brought together a dozen representatives from state and party agencies involved in the genocide of Europe's Jews. The notoriety of the meeting stems in large part from the fact that a summary of the proceedings—the so-called Wannsee *Protokoll* authored by Adolf Eichmann, who was also present—survived the war. Historians have used the document to implicate a broad spectrum of German bureaucracies in the mass murder, to demonstrate the leading role played by the SS, and to underscore the cold premeditation with which the killing was conceived and planned.

The significance of the meeting, however, remains a matter of some disagreement among scholars of Nazi Germany who have been involved in a broader debate

about the origins of the "Final Solution." Christian Gerlach, for example, has recently suggested that the prevailing interpretation of the conference as a discussion of logistical details actually underestimates the importance of the event. Gerlach argues instead that the conference must be understood as having been closely connected with Adolf Hitler's decision to extend the genocide from Eastern European Jews to Jews throughout Europe, a decision that Gerlach dates to just a few weeks before the conference (see Christian Gerlach, *Krieg, Ernährung, Völkermord: Deutsche Vernichtungspolitik im Zweiten Weltkrieg* [1998], chapter 2).

One reason historians disagree over such issues is that the *Protokoll* itself is a deliberately vague summary of the conference rather than a precise transcript of the discussions. Eichmann drafted the document to privilege the interests of the SS, and specifically the RSHA, for which he worked; he referred to the mass murder of the Jews only euphemistically. For the purposes of making a film, then, the *Protokoll* provides a plot summary but not a script.

In translating the document into a film, therefore, the makers of *Conspiracy* took certain liberties. Cinematic license is most conspicuous when the film presents conversations that are mentioned neither in the *Protokoll* itself nor in related documentation. One tendency of these embellishments is to overstate the degree of disagreement and dissent at the conference, elevating what were most likely technical concerns into moral objections. For example, Friedrich Wilhelm Kritzinger, representative of the Reich Chancellery, is depicted as a bold dissenter, protesting to the meeting that Hitler had personally assured him that killing of Jews would not be the state's policy. There is no documented instance, however, of Kritzinger actually having said this. It is likely that this fictitious comment was inserted into the script to reinforce the dramatic function assigned to Kritzinger in the film, namely that of moral dissenter, a role which he, in actuality, did not play at the conference.

Similarly, the film shows Heydrich working behind the scenes, pressuring and cajoling Kritzinger and Wilhelm Stuckart of the Interior Ministry, ultimately securing the assent of both men to his point of view. We do not know whether these private conversations really occurred. Heydrich is also depicted asserting to his listeners that the labor mobilization of Jews would merely serve as a cover for their liquidation. But both the *Protokoll* and the reality of Nazi measures at the time suggest a more complicated relationship between forced labor and extermination.

Conspiracy will naturally be compared to the German film *Die Wannseekonferenz* (dir. Heinz Schirk; 1984), a similarly conceived docudrama based on the *Protokoll*. The earlier German film conforms to the text of the *Protokoll* more closely, but this does not necessarily mean that it is more historically accurate. For example, *Conspiracy*'s most dramatic deviation from the *Protokoll* is its presentation of a discussion of

the murder process, including the nascent technology for mass gassing and cremation. This addition, however, is justified. In his interrogation by the Israelis in 1960, Eichmann admitted that such a discussion had indeed taken place, but he had deleted it from the official summary.

Jointly underwritten by HBO and BBC, *Conspiracy* features high production values and well-known actors. Kenneth Branagh dominates the film with his characterization of Heydrich. Branagh evokes the cultured yet swashbuckling image that Heydrich cultivated, so much so that I wonder whether the film makes Heydrich into a more compelling figure than he actually was. Moreover, Branagh comes off as seeming a good deal older than Heydrich, who was thirty-seven years old when he presided at Wannsee. Stanley Tucci's interpretation of Adolf Eichmann as a proactive anti-Semite represents a refreshing departure from the old, and inaccurate, cliché of the "banal" bureaucrat.

"This film," we are informed in the closing credits, "is based on a true story, with some scenes, events, and characters created or changed for dramatic purposes." Although academic specialists will doubtlessly be perturbed by inaccuracies and interpolations, *Conspiracy*, to its credit, does not stray very far from what is factually plausible. The main danger with this kind of film is that most viewers will not be able to tell the difference between plausible speculation and documented fact.

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LUMUMBA. Produced by Jacques Bidou; directed by Raoul Peck; screenplay by Raoul Peck and Pascal Bonitzer. 2000; color; 115 minutes. France (French, Belgian, German, and Haitian coproduction); French with English subtitles. Distributed by Zeitgeist Films.

In 1961, shortly before his assassination, newly elected Congolese Prime Minister Patrice Lumumba predicted: "History will one day have its say, but it will not be the history that Brussels, Paris, Washington or the United Nations will teach." Three decades after Lumumba's untimely death, Raoul Peck has created a powerful testament to the possibility of a history as written by the vanquished. Peck has said: "This film is not an 'adaptation,' it aims to be a true story. I want to extract the cinematic narrative from reality by remaining as true to the facts as possible." With an eye for authenticity and an urge toward documentation, Peck attempts to rescue the story of Lumumba's brief, explosive political career and its attendant legacy from its relative obscurity. For Peck, this represents more than a simple retelling of the historical facts from a new perspective. Audiences familiar with the history covered in the film will recognize many of film's pivotal scenes as living recreations of famous still photographs and newsreel footage from Lumumba's short political

life and assassination. In *Lumumba*, Peck is doing far more than simply recreating Lumumba's story and Congolese history for a generation and a wider audience that may or may not be familiar with it. Instead, he is using the debris of official history to craft a meditation on the early postcolonial moment in a way that gives a new significance to the events surrounding Lumumba's murder. His documentary-style recreation of authentic still photographs and newsreel footage is alternately both arresting and dynamic, as he takes hold of the historical record and recreates it as a brilliant meditative lament for what might have been.

When Lumumba died in 1961, the innocence of the early postcolonial moment largely died with him. With *Lumumba*, Peck has managed to produce a film that celebrates the innocence of that moment at the same time that it laments its loss. "Yes, I, too made mistakes and it would all end badly," intones the film's voice-over, which we are meant to take as Lumumba's after-death reflections, "We thought we controlled our own destiny but other powerful enemies were pulling the strings." As a postcolonial lament, *Lumumba* is very much about what liberation has cost and will continue to keep costing people of African descent at home and abroad. "You won't tell the children everything will you? They wouldn't understand. Don't tell them that. Say nothing," the voice-over instructs an unnamed listener as the soldiers dismember Lumumba's body.

In his final letter to his wife, Lumumba anticipated a day in which "Africa will write its own history, and it will be, to the north and south of the Sahara, a history of glory and dignity." That fabled day of African liberation has clearly not arrived, although the Belgian government recently issued an official apology for its role in Lumumba's death. What the film does, then, in memorializing the long-forgotten voice of pan-African liberation, is to bring viewers who are both familiar and unfamiliar with that dream directly back to a moment when it still seemed a not-too-distant possibility. The film's ultimate brilliance lies in its ability to do this at the same time that it calls viewers not only to question what they already know about Lumumba, Congolese history, and the early postcolonial moment but also to query the construction of history itself as a presumably static or neutral act.

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NO MAN'S LAND (*Nikogarsnja zemlja*). Produced by Frédérique Dumas-Zajdela, Marc Baschet, and Cédomir Kolar; written and directed by Danis Tanović. 2001; color; 98 minutes. Slovenia; Bosnian, Serbian, Croatian, French, and English dialogue. Distributed by MGM/United Artists.

Danis Tanović, a brilliant documentary filmmaker from Bosnia and Herzegovina, focuses on the plight of three soldiers trapped in a trench midway between

enemy lines in *No Man's Land*, his first feature film and winner of the Cannes Film Festival prize for best screenplay, a Golden Globe award, and the Academy Award for best foreign film. The director's experience filming on the front lines of Sarajevo for the archives of the Bosnian army prepared him well for this distinctly Balkan cinematic experience of ironic absurdism and haunting despair, developed in his beautifully calibrated screenplay.

A superbly acted morality tale, the film's intense realism enhances its irresistible comedic moments. Two wounded soldiers—Čiki, an armed Bosnian Muslim (played by Branko Djuric), and Nino, a Serb recruit (René Bitorajac)—are trapped in a trench when Nino's commander places a body, presumed dead, over an American-made "bouncing mine," an insidious device designed to explode a few feet off the ground and blow up everything in its vicinity. But Čera (Filip Sovagovic), a compatriot of Čiki's, unexpectedly begins to show signs of life, complicating the stand-off and creating a dilemma: the mine could kill both Čiki and Nino if Čera is moved. Despite their differences, they must work together to keep him immobilized, awaiting the arrival of a German mine expert.

In this deliberately emblematic confrontation, the antagonists hurl mutual accusations back and forth about the genesis of the war until the Bosnian forces the Serb at gunpoint to confess aloud that the Serbs were responsible. As Čiki continues to give orders, Nino demands to know why. Čiki yells: "Because I have a gun and you don't," summarizing concisely the power relations at issue here. When their roles are reversed, Nino, now in control, demands Čiki's admission that it was after all the Bosnians who were to blame for the conflict. The writer/director's deft narrative structure makes the audience experience the protagonists' escalating anger, frustration, and tension, leavened though they may be by humorous touches. The film thus encourages reflection on the origins, circumstances, and historical interpretations of this interethnic saga.

The scope of the film's layered narrative widens beyond the inextricable imbroglio in the contested terrain of the trench when United Nations humanitarian forces are alerted. Arriving in a white UNPROFOR tank, their inaction earns them the locals' disdainful appellation of "shtroumps." The British general (Simon Callow), obsessed with protecting the image of the U.N., thwarts the attempts of a French sergeant (George Sotiadi) to resolve the situation. Tanović's usually even-handed yet merciless gaze nonetheless leads him to take aim against aggression:

"You can't stay neutral in face of a murder," says a French soldier. The director's canvas transforms a small incident into a powerful metaphor, a microcosm of the Bosnian conflict in particular and war in general.

Further still from the trench are the media, embodied by a global television network that has been surreptitiously monitoring U.N. radio communications. Their video crew, led by an arrogant British journalist (Katrin Cartlidge) who threatens to expose the U.N. to promote her own agenda of getting the story at any cost, is mocked for exploiting suffering in their heartless quest for media ratings. Tanović's confident satire implicates both the farcical efforts of the U.N. peacekeepers and the machinations of media companies that make pawns of the wounded soldiers in the trench.

Woven through the film like an inside joke are the protagonists' difficulties in communicating with one another, making the multiplicity of languages spoken, and the inability to make themselves understood, a metaphor of the war itself. As the director states, "The language spoken in the film by the protagonists is in fact the same language. Today, the Serbs call it Serbian, the Bosnians call it Bosnian, and the Croats call it Croatian. The fact is that when we speak, we understand each other perfectly." By transforming a small incident into a powerful metaphor—a microcosm of the Bosnian conflict in particular and war in general—the film's true enemy becomes the kind of cynical inaction that has typified outsiders' response to the conflict, and which Tanović presents as tantamount to taking a position.

When *No Man's Land* opened the seventh Sarajevo International Film Festival, the director, anxious to fulfill the hopes of the hometown audience of 2,700 who were most important to him, was told that this was the film they had been waiting for, the authentic expression of their experience. A Bosnian-Belgian-Italian-Slovenian coproduction filmed in the lush pastoral beauty of a Slovenian setting that contrasts grotesquely with the human struggle in its midst, *No Man's Land* is not a perfect film; in the director's urgency to explain the trajectory of the Bosnian conflict, he deploys news footage in ways that tend to obscure rather than illuminate the narrative. Yet even such awkward moments fail to undermine the film's intelligent compassion, exemplified in the profoundly moving final image of Čera stranded alone on the mine.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed.

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ARTICLES

TO THE EDITOR:

Alfred J. Rieber's "Stalin, Man of the Borderlands" (AHR 106 [December 2001]) is insightful and interesting. But on page 1662, it states that Stalin's "first born, Iakov (Jacob), was named for the son of the biblical Joseph." Shouldn't that be "the *father* of the biblical Joseph?"

RON CANTOR
*Jefferson Community College,
Watertown, New York*

ALFRED J. RIEBER REPLIES:

Ron Cantor is correct, of course. Jacob was Joseph's father and not his son, and I regret the slip that inverted the relationship between them. My error does not, however, invalidate the point I was trying to make. Namely, Stalin selected a name for his son that expressed a filial relationship within the Old Testament tradition, which reflected, it must be assumed, his wife's religious outlook and his own training in the Tblisi Seminary.

ALFRED J. RIEBER
Central European University

REVIEWS OF BOOKS

TO THE EDITOR:

In his review of Filippo Sabetti, *The Search for Good Government: Understanding the Paradox of Italian Democracy* [AHR 106 (October 2001): 1498–99], Joseph

LaPalombara writes that the Milan magistrates investigating corruption in the early 1990s "not only created a political power vacuum with their so-called 'clean hands' exposés of corruption, they themselves have tried to fill that power vacuum as well, sometimes with frightening success." This statement leaves much to be desired as a description of recent Italian events. If by power vacuum, LaPalombara means the collapse of the Christian Democratic and Socialist parties in 1992–1993, this was brought about largely by public revulsion against the corruption of those parties, which (though downplayed by some) had flourished during decades of uninterrupted power. After 1989, the anti-communist parties became vulnerable to judicial investigations into their activities of the kind that were practically inconceivable during the Cold War. They were also vulnerable to a potent new protest movement, the Northern League. That a group of magistrates were (and are) conspiring to run the country against the will of the people is a typical charge made by former supporters of the Christian Democrats and Socialists. The vacuum on the center right has not, however, been filled by the so-called "red togas." That task has been ably performed by Silvio Berlusconi, who founded the Forza Italia ("Go Italy!") Party in 1993. Berlusconi, the owner of three television networks (and a defendant in several ongoing trials), is now prime minister and foreign minister of Italy.

JOHN L. HARPER

The Bologna Center of the Johns Hopkins University

JOSEPH LAPALOMBARA REPLIES:

John Harper is correct: it is indeed Silvio Berlusconi, and not the "red togas," who has filled the vacuum created by the "clean hands" investigations. The "public revulsion" he mentions, however, would never have reached the levels it did were it not for the not always admirable behavior of the magistrates. Some of the latter actually ventured far abroad to describe their own heroics. Willfully or otherwise, they gave Italy a big and undeserved black eye.

This time around, and in contrast to their erosive work of 1993–1994, the magistrates face tougher sledding. Berlusconi's parliamentary majority is solid. His

political opponents, aided by partisan magistrates, will not find it as easy to reverse by dubious political maneuvering what the voters have now twice mandated at the polls, namely, that Italy should be governed by the center-right.

Politicized magistrates, of course, pre-date Berlusconi. About this aspect of the Italian polity, I wrote in my *Democracy, Italian Style* (1987, p. 228), "in recent months the cry has been raised that the judicial arm, even when turned to the best of causes, is actually in the narrow service of one political party or another. It has also been charged with the flagrant use of the judicial power as a weapon against political opponents."

In the ensuing years, the situation has worsened. Magistrates have pursued apparent political vendettas, not just against Berlusconi but, even more shamefully and notoriously, against Giulio Andreotti, seven times Italy's prime minister. But these number only among the most prominent abuses.

Civil libertarians would find hair-raising the powers of Italian magistrates and their misuse. People are selectively arrested at their discretion. Magistrates have frequently leaked to the press the names of those who receive the infamous *avviso di garanzia*. This letter is nothing more than a simple and required notice that a magistrate is contemplating a possible investigation of the person addressed. In the "clean hands" years, thousands of these *avvisi* were mailed, most of which came to naught. Nevertheless, this flood of innuendo destroyed reputations. It also created the false impression that most of these persons had already been caught red-handed in acts of political corruption. The Italian and the world mass media gorged on such sensationalism.

Magistrates also practice de facto preventive detention, a police power that is anathema to advocates of procedural civil rights. In theory, Italians arrested must either be charged with a specific crime or released after 120 days. In practice, inventive magistrates, with the concurrence of only one other of their brethren, can (and do!) start the incarceration clock running again and again—on the vague and flimsy claim that the person so released might "contaminate the evidence."

The magistrates publicly and ferociously oppose all efforts to separate by law their powers to investigate from their powers to judge. In the United States, the

analogy would be public prosecutors who are also empowered to judge those whom they have succeeded in having arrested! The Berlusconi government, along with many others, wants this situation corrected. Toward these efforts, a leader of the Milan Pool, fully draped in the toga and other symbols of his judicial authority, has recently urged Italian citizens to "Resist! Resist! Resist!"

Such political incursions by some magistrates are now so dramatic, even a few prominent leaders of the Left have come out in favor of reform. One does not have to be (as John Harper ungenerously implies) a former supporter of Italy's discredited Christian Democrats or Socialists to condemn these judicial excesses. That Silvio Berlusconi, or for that matter any other Italian citizen, may require official investigation is no reason to continue to treat the magistracy as a sacred cow.

Harper is perhaps also correct in his surmise that wearers of the "red toga" do not conspire "to run the country against the will of the people." But, precisely because so many of them seethe with political bias and ambition, more of the aggressive magistrates might be encouraged to follow in the footsteps of Antonio Di Pietro. This most prominent of the "clean hands" magistrates courageously resigned from the magistracy, ran for and won public office, and has now formed a new political party. In short, unlike his former colleagues, he no longer hides his political agenda behind the increasingly transparent mask of judicial objectivity.

Italy's political institutions and, indeed, Italian democracy, will be on surer footing if the much-needed reforms of the magistracy materialize soon.

JOSEPH LAPALOMBARA
Yale University

ERRATUM

In the review of *Me You Them* (*Eu, Tu, Eles*) and *Maids* by Darién J. Davis, *AHR* 107 (February 2002): 322–24, the name of the first film was accidentally omitted at one point (323, col. B), leaving a blank space, and the words "activities" and "related" were rendered as "ctivities" and "elated." The editors regret these errors.

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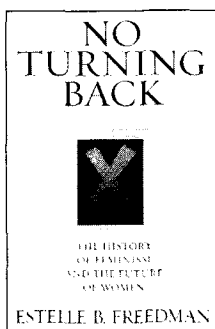
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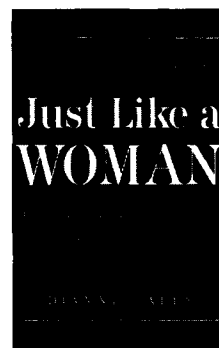
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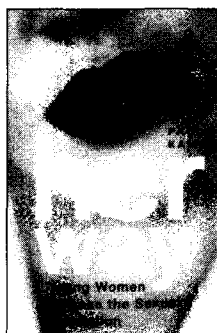
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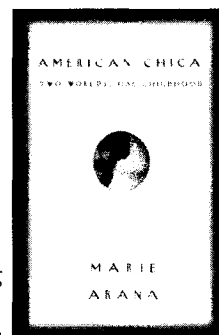
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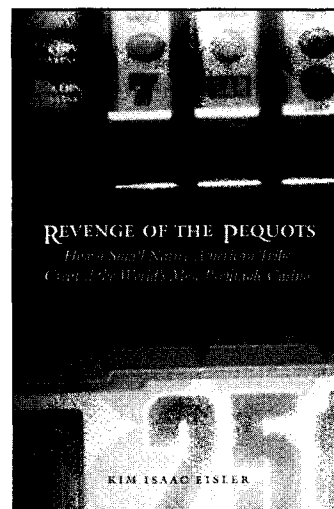
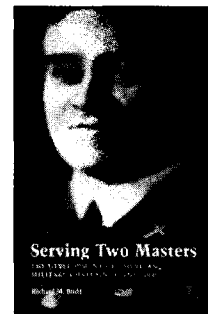
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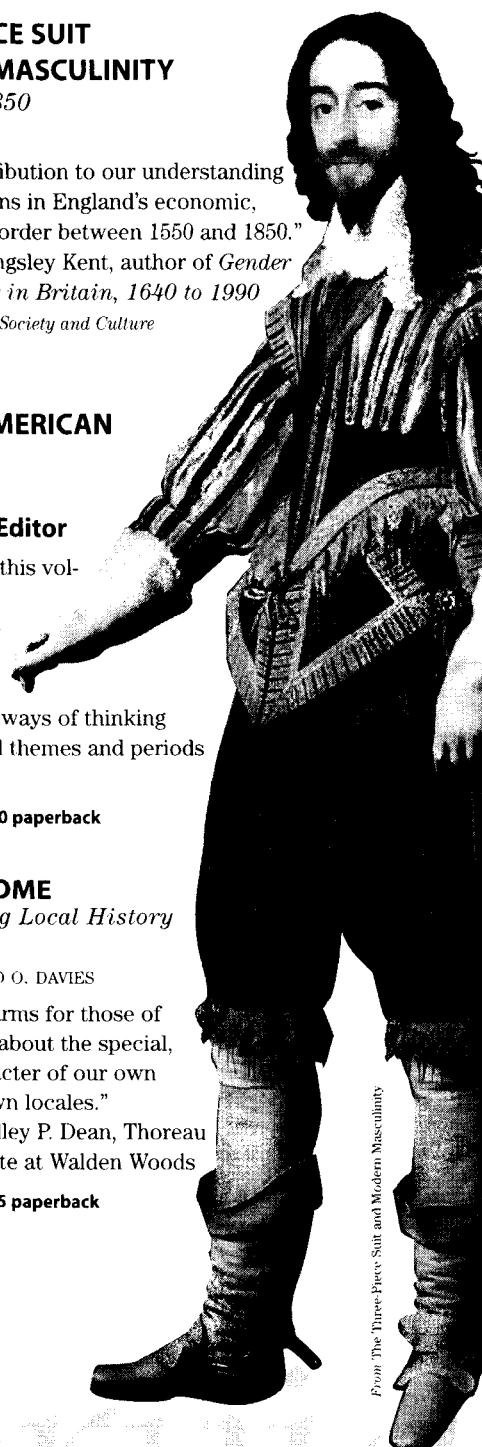
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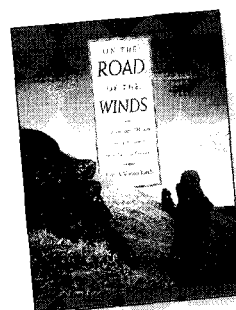
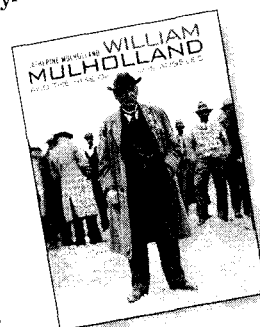
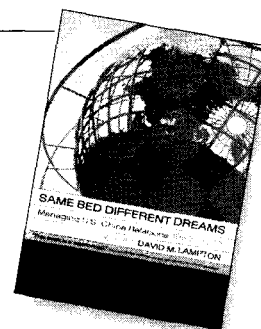
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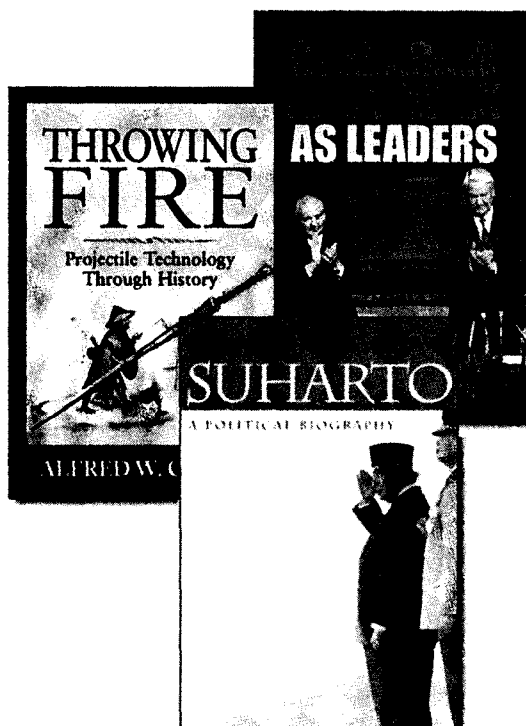
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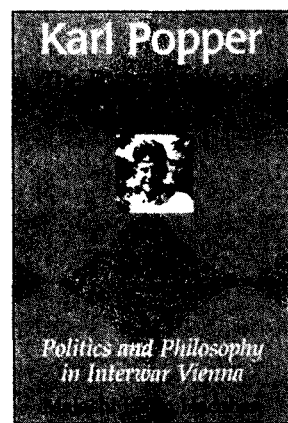
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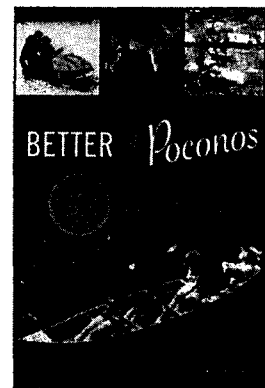
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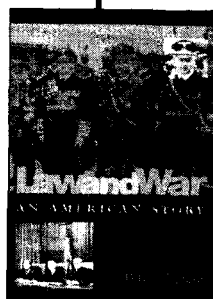
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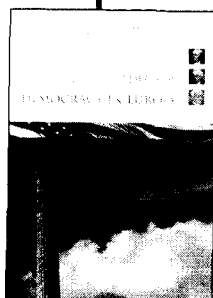
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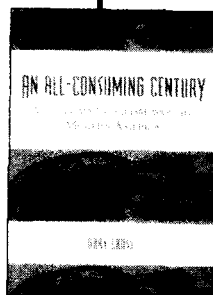
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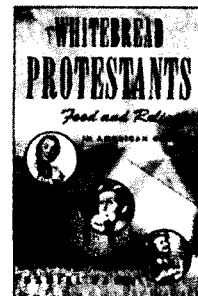
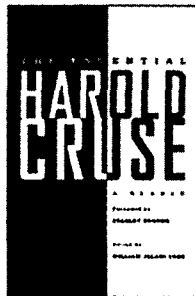
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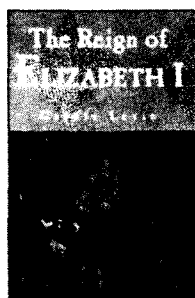
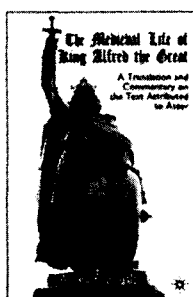
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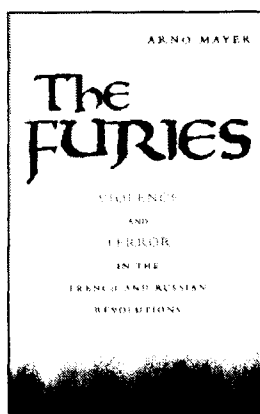
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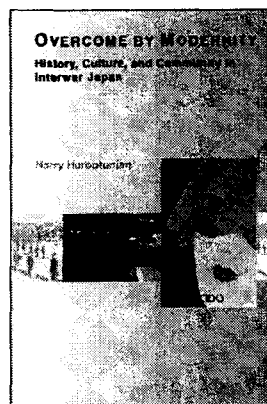
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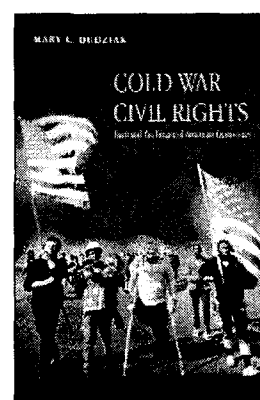
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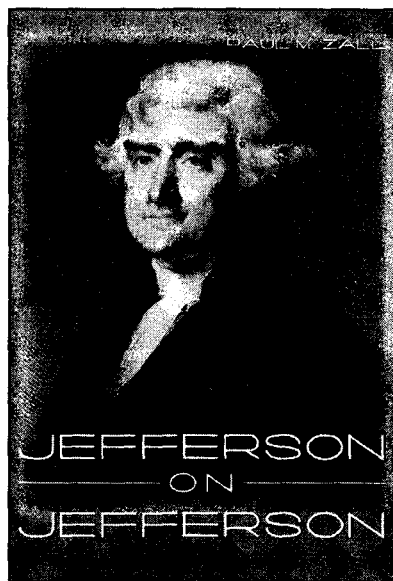
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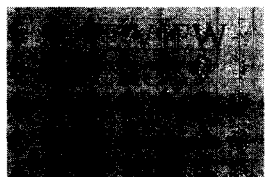
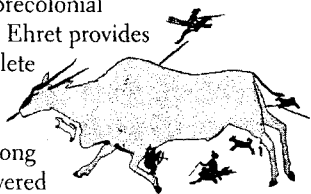
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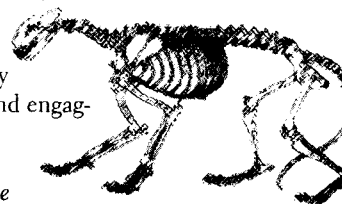
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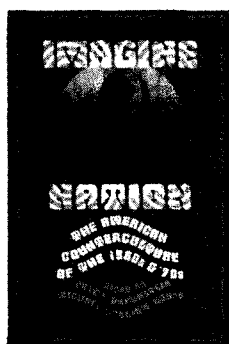
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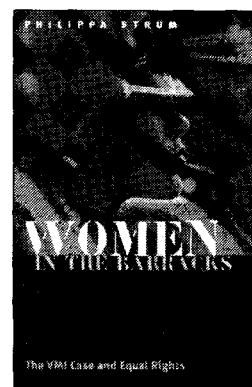
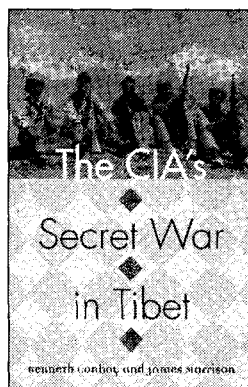
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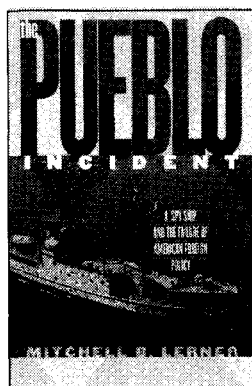
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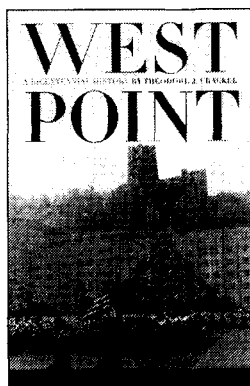
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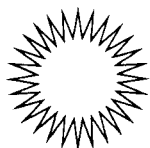
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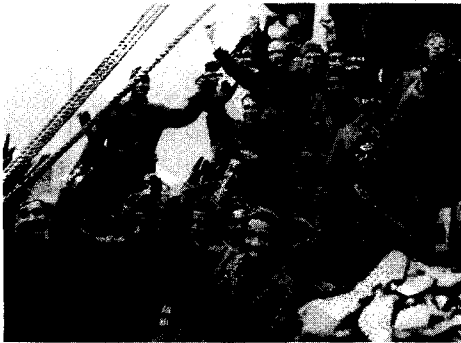


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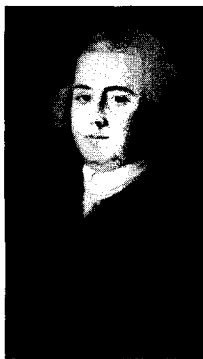
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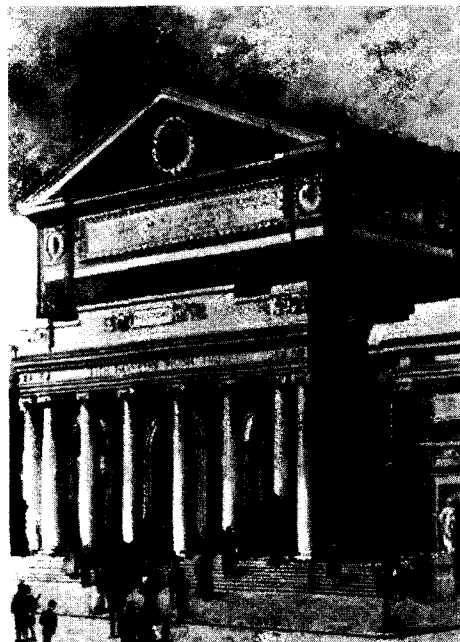
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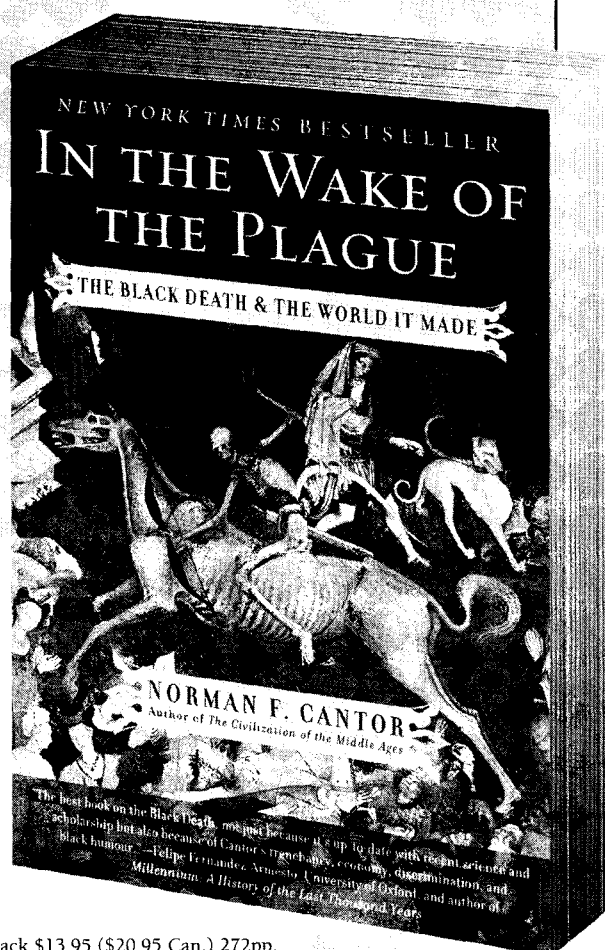
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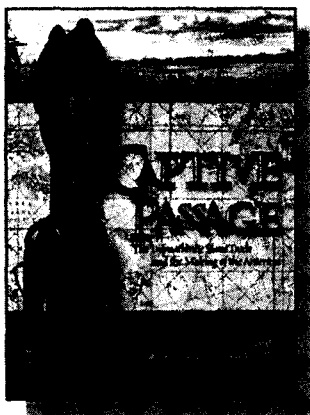


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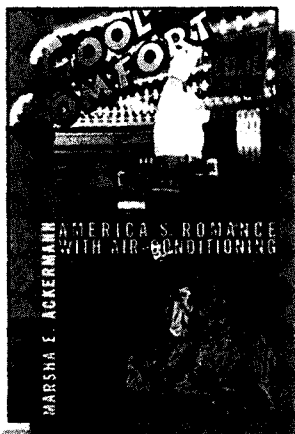
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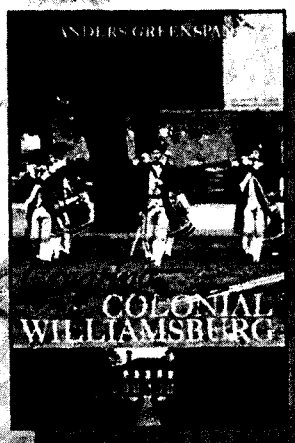
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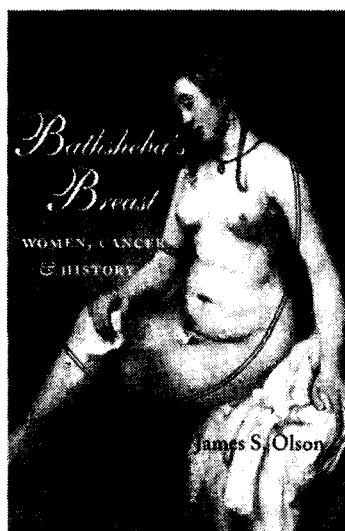
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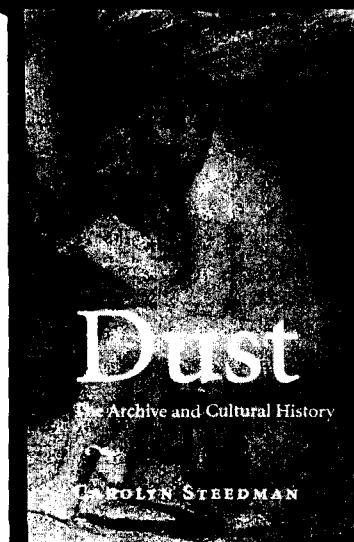
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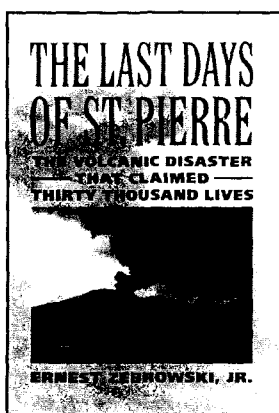
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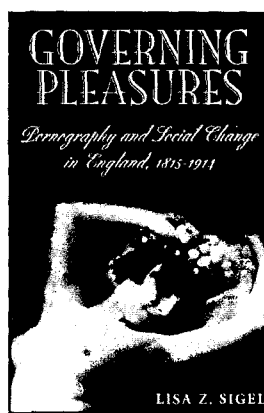
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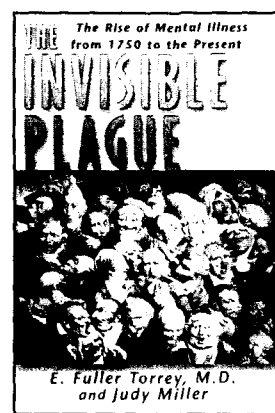
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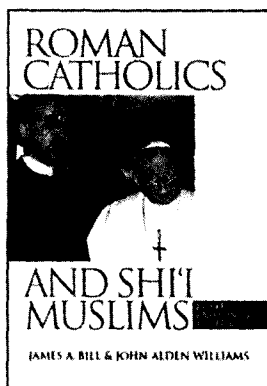
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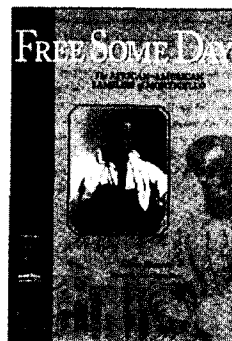


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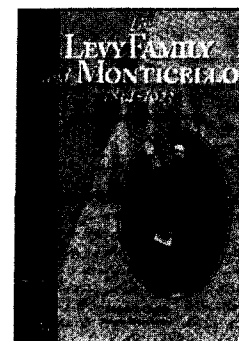
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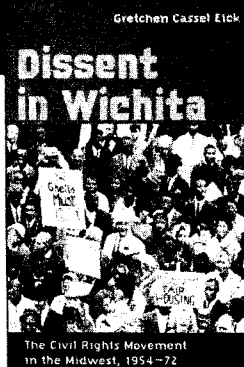
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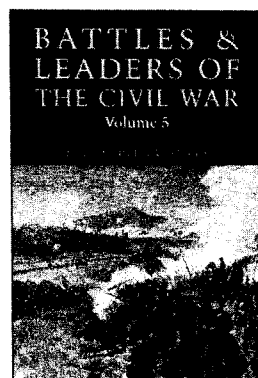
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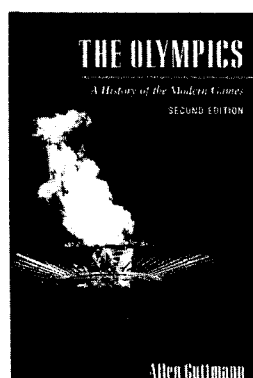
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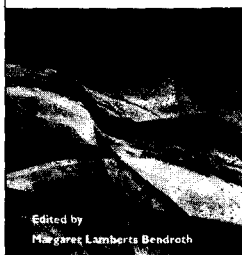
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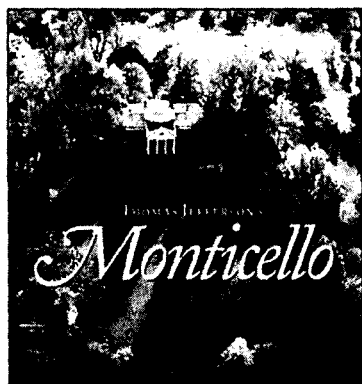
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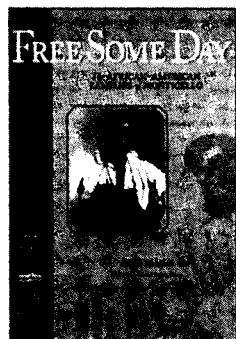


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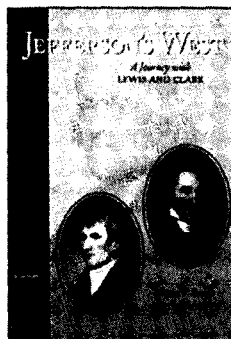
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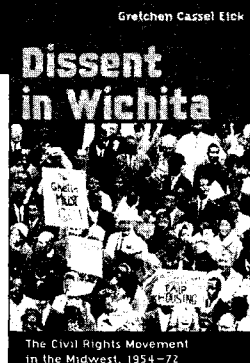
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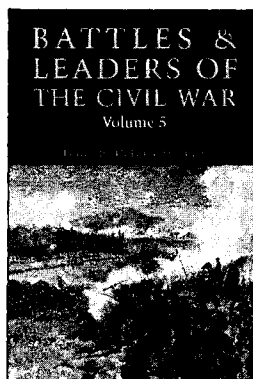
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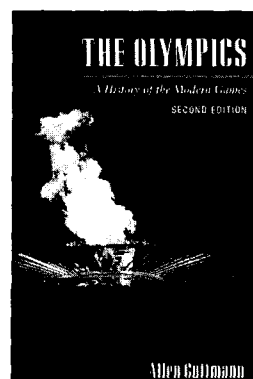
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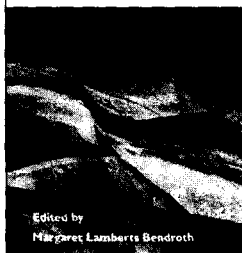
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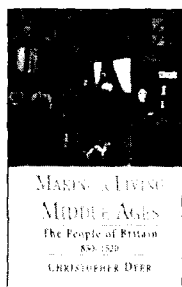
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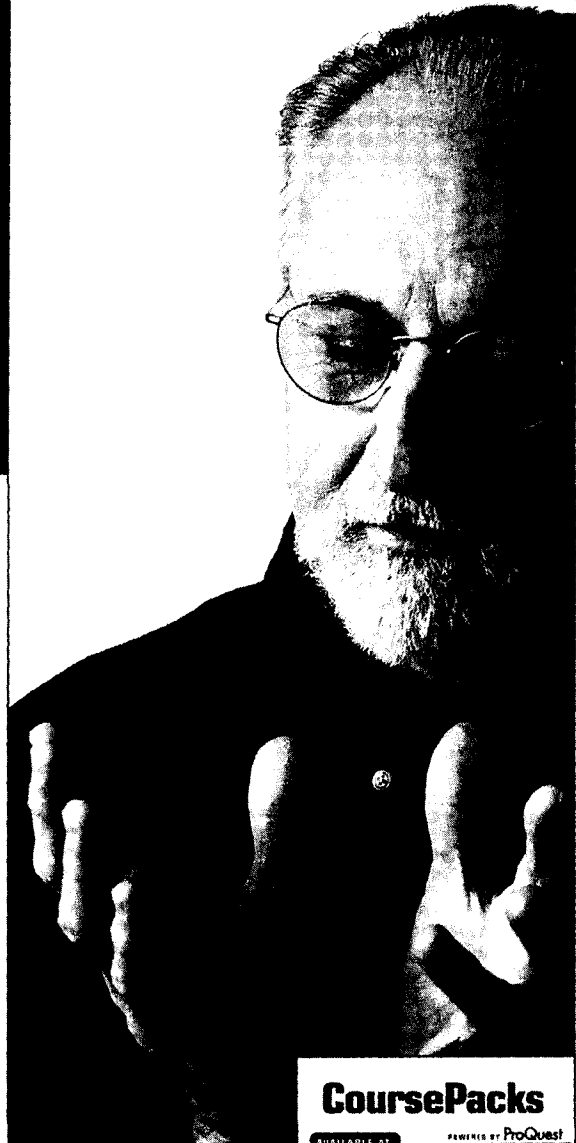
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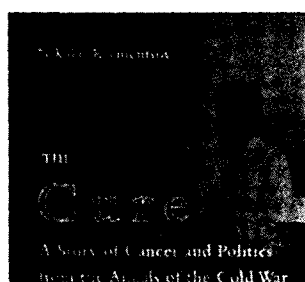
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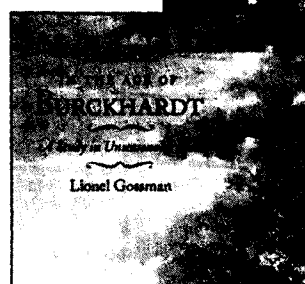
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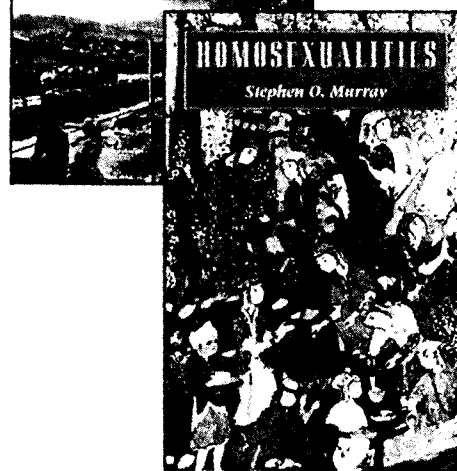
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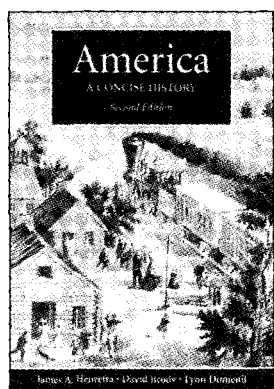
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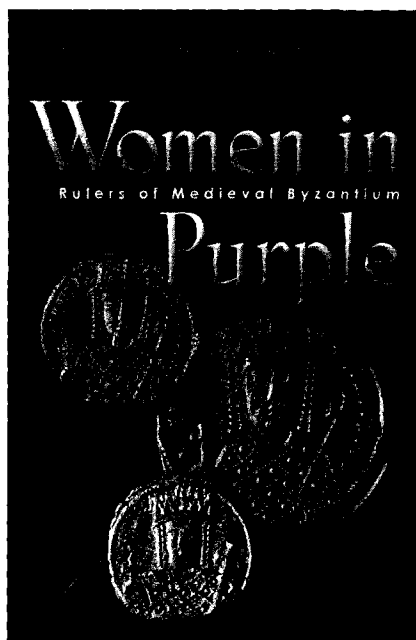
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